

# TALKING ABOUT WHAT THERE IS

*Eli Hirsch's Attack on Ontology*

*By*

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## ABSTRACT

Ontology attempts to answer the question “What is there?” Trying to pursue this question, contemporary analytic philosophers argue over whether there are tables over and above particles arranged tablewise, whether ordinary objects persist through time by having instantaneous temporal parts and whether particles can compose both a lump and a distinct statue at the same time. The disputes are highly technical and contrived, yet ontologists carry on. But there is trouble in paradise. So-called ontological deflationists attempt to undermine the disputes by arguing that the disputes are insubstantial; they are not about the world, but rather about the correct use of language. Eli Hirsch is one of the philosophers who have challenged ontology, and his flavor of deflationism, quantifier variantism, says that there are many ontological languages with different answers to the ontological question. Ontologists merely disagree about which language to speak – they are engaged in a verbal dispute. Furthermore, ontology conducted in English (or any other natural language) is pointless, for the ontological beliefs of ordinary people – their beliefs in the existence of tables and chairs, statues and lumps – are trivially true. In sum: either ontologists speak different languages, in which case their ontological “theses” will merely reflect their linguistic choice, or they speak English, in which case any “results” will be trivial. In any case, ontology as we know it is undermined.

This thesis explores Hirsch’s view in more detail with the purpose of critically examining his arguments. I provide two main lines of argument against Hirsch: (1) The idea of different ontological languages is more radical and problematic than it is usually assumed, potentially undercutting the philosophical significance of Hirsch’s arguments; (2) Commonsensical ontological claims are not trivially true in English, and it might be possible to conduct ontology in English after all. I also consider a recent reply to Hirsch by Theodore Sider to the effect that ontologists should leave English for a dedicated ontological language. I show how this reply will rehabilitate ontology, but argue that it is not entirely successful.

The present work does not provide a full-fledged defense of ontology, but tries to answer one influential criticism in illuminating detail.



## PREFACE

The following concludes four years of joy and frustration exploring metaphysics. At the beginning of my studies in philosophy I found metaphysics puzzling, difficult, enthralling. Only later did I understand the problems underlying it. This thesis grapples with some of them.

I want to thank my supervisor Associate Professor Einar Duenger Bøhn for penetrating comments and enlightening discussions on the topics discussed herein over the last year and a half. Professor Matti Eklund has also been very helpful in answering my pondering e-mails.

Many have directly or indirectly contributed to this work. I have benefited greatly from discussions with the closest I come to intellectual brothers, Reier Helle and Jørgen Dyrstad, and they have read and commented on significant parts of this thesis. I also want to thank Carl Martin Rosenberg and Ludvig Fæhn Fuglestad for reading through much of the material and providing helpful comments. The support from friends I've met during my five years and counting at the University of Oslo has been important for the motivation necessary to write this thesis. Thanks to Ingild for love, devotion and an occasional motivational interview.

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## INTRODUCTION

In a paper that has been credited with reviving ontology in analytic philosophy,<sup>1</sup> Quine claims that the ontological question is “What is there?” (1948, p. 21). In the next breath, Quine says that everyone agrees that the correct answer is “everything”, but that there is room for disagreement about cases (1948, p. 21). Since then, and with the important contribution from philosophers such as Chisholm, Armstrong and Lewis, considerable disagreement over cases has ensued. Ontology has overcome its troubled past<sup>2</sup> and become a thriving research program. Not only have philosophers caught up on traditional questions about the existence of universals, God and numbers, but the revival of metaphysics has also made way for new questions, some more parochial than others. To the uninaugurated, the dispute over the existence of macroscopic composite objects such as tables and chairs will perhaps seem silly, and non-philosophers will probably not share philosophers’ qualms about believing in the existence of both copper statutes and lumps of copper. Lack of understanding from ordinary people has seldom stopped a philosopher’s theorizing, however.

Ontologists in the tradition of Quine – which is the tradition I will be concerned with – consider ontological disputes as substantive and about objective reality; settling them requires complex arguments and theoretical sophistication. Theodore Sider describes the methodology for answering ontological questions thus:

Competing positions are treated as tentative hypothesis about the world, and are assessed by a loose battery of criteria for theory choice. Match with ordinary usage and belief sometimes plays a role in this assessment, but typically not a dominant one. Theoretical insight, considerations of simplicity, integrations with other domains (for instance science, logic, and philosophy of language), and so on, play important roles. (Sider 2009, p. 385)

Manley (2009, p. 4) characterizes this as *mainstream ontology*, and it has been flourishing for the last decades. But lately, ontology has come under attack. So-called deflationists argue that ontological disputes are in some way or other misconceived. The diagnoses of the malady differ, and the exact disputes the criticism is targeted at varies,<sup>3</sup> but deflationists have in com-

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<sup>1</sup> Putnam claims that Quine “single-handedly made Ontology a respectable subject” (2004, p. 78).

<sup>2</sup> Ontology, and metaphysics in general, has been under attack in several periods of the history of philosophy. The first critics were perhaps the sophists, and Hume famously wanted to commit metaphysics to the flames (1993). In more recent times, both the logical positivists (Ayer 1978; Carnap 1950) and ordinary language philosophers (e.g. 1953) took a negative attitude towards metaphysics. As will become clear in the following thesis, also contemporary philosophers are skeptical of metaphysics. Recent criticisms can be found in Hofweber (2009), Thomasson (2007, 2009) and Yablo (2009), in addition to Eli Hirsch, whose criticism is the topic of this thesis.

<sup>3</sup> Thus Yablo thinks the dispute about the existence of numbers is insubstantial and silly, but seems more positive towards the dispute over the existence of God and theoretical posits (1998, p. 259). This can be contrasted with

mon a skeptical attitude towards ontology, believing that something's wrong with the hyper-theoretical disputes of ontology. The general attitude may perhaps be summed up by this sigh: "There is nothing at stake here!"

In this thesis, I will concentrate on the perhaps most influential and arguably best form of ontological deflationism, the *quantifier variantism* of Eli Hirsch (2011).<sup>4</sup> The ultimate goal of Hirsch's arguments against ontology is to defend a "common sense" view on what there is (2011, pp. 97–98). This is in contrast with most contemporary ontologists, who at least consider it an open question whether all and only the objects of common sense exist.<sup>5</sup> Hirsch defends a conception of ontology in which ontological disputes are "shallow" and where there is no room for the speculative debates of mainstream ontology, which Hirsch characterizes as "afflicted with a kind of hyper-theoreticalness" (2011, p. 82) that leads to "futile and interminable pseudo-theoretical arguments" (2011, p. 82). In another essay, Hirsch describes ontological questions as "laughably trivial" (2011, p. 90). According to Hirsch, ontological questions can be answered by determining which sentences about existence are true,<sup>6</sup> and the principle of charity plays a central role in this process (Hirsch 2011, p. 231). Correct linguistic interpretation of English yields the trivial falsity of non-commonsensical<sup>7</sup> existence claims, such as "There are no buildings in New York City," and the trivial truth of commonsensical existence claims, such as "There is at least one building in New York City" (2011, p. xiii). Given Hirsch's view on how ontology should proceed, mainstream ontology is misconceived.

The goal of the following chapters is to look for the best defense of ontology against Hirsch, but as the arguments turn on difficult questions in adjoining fields, especially the philosophy of language and epistemology, there is little hope of a defense that will satisfy all philosophers. Nonetheless, there is a virtue in trying to better understand the challenges facing ontology, for in the light of the recent criticism, ontology cannot continue untouched. The goal is primarily negative: it is to show that Hirsch's arguments don't undermine mainstream ontology, not to give an independent justification of why the method of mainstream ontology is adequate for ontology.

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Thomasson, whose focus is squarely on the dispute over ordinary objects. Hirsch thinks all disputes about the nature and existence of macroscopic objects are trivial, see section 1.3.

<sup>4</sup> Referencing Hirsch's work poses a challenge. His papers on metaontology are collected in Hirsch (2011), and thus it is convenient to use it as a common source of reference. On the other hand, it is sometimes necessary to reference his papers as a whole. I've solved this by citing individual papers by the year they were published, but direct quotes and passages are referenced by their page-number of the essay in Hirsch (2011).

<sup>5</sup> An example of a defense of "common sense" ontology within mainstream ontology is Markosian (1998).

<sup>6</sup> Hirsch's metaontological arguments are of a linguistic nature. Hirsch (2011, p. 98) expresses certain sympathies for so-called "Moorean" epistemic arguments for common sense ontology, but believes that the linguistic arguments discussed in this thesis will be harder to ignore for ontologists. I agree.

<sup>7</sup> I will talk about ontological claims such as "There are no tables" and "There is an object that has the Eiffel tower and Plato's nose as parts" as "non-commonsensical" or "revisionary" ontological (or quantificational or existence) claims.

## Quinean Metaontology

This is a thesis in what has recently been known as *metaontology*. While ontologists ask “What is there?” and argue about whether there are numbers or temporal parts, metaontology deals in meta-questions such as “What are we asking when we ask ontological questions?”, “Are there objective answers to the ontological question?” and “How do we know what there is?” In this regard, metaontology is similar to metaethics, and thus ventures into other sub-disciplines of philosophy for answers.

Philosophy is a diversified field with little consensus, so one must make certain assumptions and choices before moving on to arguments. By focusing on Hirsch’s attack on ontology, I have by implication chosen to focus on what’s often called Quinean ontology, because Hirsch’s arguments are most directly targeted at Quineans. Besides an interest in the challenges Hirsch has posed, the reasons for choosing to defend this specific framework is that I consider it the most promising and interesting “framework” for conducting ontology. It is also presently the sociologically most dominant one,<sup>8</sup> and a Quinean metaontology is presupposed by much recent work in ontology.<sup>9</sup> Mainstream ontology is usually assumed to presuppose a Quinean metaontology, which makes a defense of it all the more pressing if one cares for mainstream ontology.

Quinean metaontology doesn’t necessarily correspond to Quine’s specific theory of ontology. Contemporary philosophers have rejected aspects of his metaontology, and many have departed from Quine in different ways. My preferred condition for understanding a dispute as conducted within a Quinean<sup>10</sup> framework is that it focuses heavily on the truth of existentially quantified sentences.<sup>11</sup> Quine writes that: “If we affirm a sentence governed by ‘something’ there had better be an object in our universe that meets the condition that the sentence imposes” (1984, p. 17). What Quine has in mind is that something has to exist for a “there is”- or “exists”-sentence to be true.<sup>12</sup> This criterion does a good job at distinguishing what intuitively

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<sup>8</sup> Chalmers, Manley and Wasserman’s (2009) consist of many of the most important new metaontological essays. The wide majority of the essays in that anthology take *something like* a Quinean view as presumption, though they diverge in quite significant ways in their overall metaontology. Thus Fine (2009, p. 157) writes that Quinean metaontology is “accept[ed] in one form or another by all of the other contributors to the present volume”.

<sup>9</sup> Sider (2009, p. 169) writes that “recent work on ontology nearly always relies on the Quinean methodology”.

<sup>10</sup> One might want to call mainstream ontology “neo-Quinean”. I won’t take a stance on which name is more appropriate; that depends on an exegesis of Quine’s view compared to mainstream ontology, which is a task I won’t pursue because it is largely immaterial to the present topic.

<sup>11</sup> This is especially intended to exclude theories that use different ideology to frame ontological questions. For instance, Schaffer (2009) takes metaphysics to be concerned with “What grounds what”, and thus uses “x grounds y” as the central ideology. Fine (2001, 2009) appeals to an “in reality”-operator to frame ontological questions.

<sup>12</sup> I shift unproblematically between the existential idioms “there is” and “exists”. It is a common assumption in contemporary analytic philosophy, explicit in Quine and an integral part of Quinean metaontology, that these expressions mean the same. Even though a minority of contemporary philosophers disputes this, early on Alexius Meinong (1960), more recently Parsons (1980) and Fine (2009), I will not distinguish between these locutions. If one wants argument, see Quine (1948) and van Inwagen (1998). See also section 1.2.4.

is a Quinean metaontology from those that are not, but in the end the relevance of my discussion for a particular philosopher's ontological aspirations depends on whether she is affected by Hirsch's arguments. Lately, several accounts on the foundations of Quinean ontology have been developed, and they provide variations on the method, epistemology and semantics of ontological disputes.<sup>13</sup> I will unfortunately not be able to discuss these different "Quinean" metaontological frameworks. Nonetheless, the Hirschian arguments would seem to affect most versions of Quinean metaontology, and thus I believe the discussion will have general relevance for Quinean ontologists.

By adopting other metaontological frameworks one can bypass Hirsch's challenges, but none of these alternative metaontological frameworks will be discussed here. In the scope of this thesis, I can only do the Quinean framework justice. As I said, one must make choices.

### *Roadmap*

The thesis has four chapters. The first chapter lays out Hirsch's arguments, focusing on two arguments and two theses that are supposed to follow from them. The theses, *quantifier variantism*<sup>14</sup> and *linguistic choice*, are both thought to have deflationary consequences. I argue that quantifier variantism consists of two claims, a weak and a strong. Chapter 1 is devoted to presentation of Hirsch's arguments, and I criticize them only to illuminate the position. In chapter 2, I evaluate the weak thesis and after defending it against some recent arguments, I argue that there is reason to believe that the thesis cannot do the work the deflationist needs it to do. I also argue that the thesis entails quite radical changes to our conception of language with problematic consequences. Chapter 3 discusses linguistic choice and argues that Hirsch has not shown that revisionary ontological claims in English are trivially false, thus indirectly I defend the contention that ontology can be carried out in English. Finally, chapter 4 discusses a recent reply to strong quantifier variantism by Ted Sider (2009, 2011). I argue that the exact way in which Sider attempts to answer Hirsch fails, but I keep it open whether the defense can eventually succeed. I briefly conclude in section 5. In sum, then, I argue that there are ways for the ontologist to avoid Hirsch's deflationary conclusions, but I do not provide a full-fledged defense of ontology. That would require a full metaontology, which is a project for another day.

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<sup>13</sup> Two different statements of consciously Quinean metaontological frameworks are van Inwagen (1998, 2009) and Sider (2009, 2011).

<sup>14</sup> This thesis has often been called "quantifier variance" in the literature, but I will follow Hirsch (2011) in calling the thesis "quantifier variantism". When I use "quantifier variance," it is to describe the phenomenon that the quantifier varies in meaning between different ontological languages, see below.

# 1 HIRSCH'S ARGUMENTS AGAINST ONTOLOGY

## 1.1 Overview

The topic of this thesis is Hirsch's arguments against mainstream ontology. The conclusions of those arguments can be put forward as two theses, both with deflationary consequences. I will give an initial characterization of them here, and then describe them in more detail later in section 1.3 and 1.4 respectively.

Hirsch presents the first thesis, which I will call *quantifier variantism*, as follows: "the world can be correctly described using a variety of concepts of the 'existence of something'" (Hirsch 2011, p. 68). We can talk about what there is in a number of quantificationally different, but descriptively equivalent, ways. A part of this thesis is that there is no quantifier that is metaphysically privileged – there is no best language to talk about which things there are (Hirsch 2011, p. xii; 84).

The other thesis is that ontological disputes reduce to linguistic choice (2011, p. xiii). Hirsch argues that either ontologists are speaking different languages, in which case their dispute is verbal, or they are speaking English, in which case non-commonsensical ontological claims are trivially false. Call the argument for this thesis *the argument from charity*,<sup>15</sup> and the thesis itself *linguistic choice*.<sup>16</sup>

Both theses are meant to show that ontology is pointless and futile, but in different ways. Quantifier variantism entails that distinct ways of describing the world in terms of the existence of things are equally good, and therefore there is no real disagreement about ontology to have in the first place. Talking about objects persisting by having temporal parts or being "wholly present" are equally good metaphysically; for the several languages one may speak, none is "nearer to reality" than the others, so there is nothing to disagree about.<sup>17</sup> The argument from charity partly presupposes quantifier variantism. It says that correct interpretation of the linguistic behavior of ontologists and ordinary people entail that either ontologists are speaking different languages (which are equally good at describing the world), and are engaged in a verbal dispute, or they are speaking English, but revisionary ontological claims are

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<sup>15</sup> Hirsch (2002b) uses this name for an argument to the effect that non-commonsensical ontological claims in English, such as "There are no tables" or "Ludwig Wittgenstein had in 1916 a temporal part that participated in the First World War" are trivially false in English. I have extended the meaning of that name a little, including also the arguments of Hirsch (2005, 2009) to the effect that ontological disputes are verbal. It is still a telling name for the argument, as both arguments rely on the same kinds of considerations of charity. See section 1.4.3 below.

<sup>16</sup> I'm following Eklund (2011) half way in labeling the two theses in this way (I use "linguistic choice" instead of his "verbal dispute"). Hirsch acknowledges that the two theses are "meddled together" in his writings on metaontology (2011, p. 212). I have tried to continue Eklund's job of disentangling the claims and arguments for them.

<sup>17</sup> The phrase "nearer to reality" is from Urmson (1956, p. 186), and is quoted in Hirsch (2011, p. xi).

trivially false. Ontological disputes are reduced to linguistic choice, which there is nothing “deep” or “theoretical” about. When we have chosen a way to talk, there is no substance left to ontology. It would seem to follow from both arguments that there is no room for the elaborate method and complicated arguments of mainstream ontology.

Linguistic choice and quantifier variantism can be construed as either local or global theses. Hirsch holds linguistic choice only locally: He believes that the dispute between endurantists and perdurantists is merely verbal, but at the same time holds that the dispute between nominalists and platonists in the philosophy of mathematics is not (2009, p. 224f). This is because the argument from charity trades on the properties of the disputes themselves; linguistic choice is a dispute-relative thesis, which Hirsch holds for ontological disputes over the nature and existence of middle-sized dry goods (2011, p. 101). Quantifier variantism, on the other hand, is on the face of it the global claim that every aspect of the world can be correctly described using different concepts of existence (Hirsch 2011, p. 68). However, Hirsch’s discussion of quantifier variantism focuses, as with linguistic choice, on the ontology of medium-sized dry goods. As I argue in section 1.3, quantifier variantism is more precisely formulated as the thesis that *certain aspects* of reality are describable with a variety of concepts of the “existence of something”, and thus quantifier variantism is a local claim in the same way as linguistic choice. Hence the example-disputes in this thesis will all concern the ontology of physical objects.<sup>18</sup>

## 1.2 Four assumptions

There are four key assumptions underlying Hirsch’s arguments. It will be convenient to have them all on the table initially to understand where Hirsch is coming from. This also makes it possible for the reader to evaluate whether she accepts them or wants to challenge them on grounds I don’t consider. Space constraints prohibit me from arguing for them in any detail, so I will be content by laying them out (Hirsch doesn’t argue for them either). I will discuss some of them critically in the chapters that follow. For now, they must simply be assumed.

### 1.2.1 *A priori* necessity

The first assumption is that ontological disputes concern matters of *a priori* necessity (Hirsch 2011, p. 222). If two ontologists disagree about what there is, they must be understood as defending their claims on grounds of *a priori* necessity. Ontological questions have answers that are *a priori* knowable, and the answers are necessary truths. This assumption figures in the

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<sup>18</sup> This does not mean that global formulations of the theses are uninteresting, but justifying it would require significantly more argument than Hirsch has provided. It should be noted that the general strategy could be extended to other disputes, but that it will be a question of sustained argument whether such a strategy will succeed in deflating e.g. the ontology of mathematics. There might be different opinions here.

argument from charity, and the claim about necessity plays a role in the argument for quantifier variantism.<sup>19</sup> This assumption will be critically discussed in section 3.3.2.

### 1.2.2 The intensional assumption

The second assumption is an intensional view on language. Consider some ways the world could have been: Al Gore could have won the 2000 presidential election; the Scottish vote on independence could have turned out differently. Possible worlds are maximally specific ways the world might have been, and Hirsch assumes that the truth-conditions of a sentence can be identified with the set of possible worlds in which it holds true (Lewis 1986). Because of space constraints, I will have to rely on the reader's prior knowledge of intensional semantics, alternatively her intuitive grasp of the concepts. I will refer to this as "the intensional assumption", and assure the reader that it won't be playing a technical role. What's important about this assumption is that it entails a specific way of individuating truth-conditions: by possible worlds. In section 1.3, I will discuss what it means for two languages to describe the world equally well, and there the claim will be that it requires that they are truth-conditionally equivalent. Given the intensional assumption, this amounts to expressing the same intensions.

### 1.2.3 The interpretation assumption

A third assumption is that interpretation is done at the sentence level, and thus that it is sentences, not terms, that primarily describe the world. Hirsch writes that the primary focus in interpretation is "always on whole sentences and how to assign truth conditions to them in the most charitable way possible" (2011, p. 150). The meaning of terms is determined by the meaning of sentences. According to Hirsch, the "essence of language is the distribution of a set of characters over a set of syntactically structured sentences" (Hirsch 2011, p. 239). Characters are functions that take a context as input and deliver a proposition for each context.<sup>20</sup> Hirsch rejects a "picture of language in which the characters at the level of sentences are generated by some underlying referential mechanisms at the level of words" (Hirsch 2011, p. 238). On Hirsch's picture of language there are no *a priori* constraints on which sentences can be associated with which characters. For instance, it is not a requirement that the ultimate explanation of the meaning of a singular term is that it refers to an object. Interpretation is association of a sentence with a character, and the principle of charity plays a crucial role in determining the interpretation of a set of sentences (see section 1.4.2).

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<sup>19</sup> This assumption is regularly made in the literature on ontology, see for instance Sider (2001a, p. 202–203); Armstrong (1997, p. 12–13); Markosian (1998, p. 216–217). An example of someone denying it is Cameron (2007).

<sup>20</sup> Hirsch has taken the term "character" from Kaplan (1989). Kaplan took characters to be functions from contexts to contents, but Hirsch talks about propositions or truth-conditions instead of content. The differences in terminology do not matter here.

Associated with this assumption is a liberal view on the individuation of languages. Let an *interpretation* of a language be a set of ordered sequences of the well-formed sentences of the target language and a character,  $\langle \text{Sentence, character} \rangle$ , for every sentence of the language. The claim is that languages are differentiated by their interpretation: distinct languages do not have the same interpretation (Hirsch 2011, p. 224). Hirsch’s view on language may be characterized as liberal because he quite freely stipulates a multiplicity of different languages based on this. All that is required for a “language” is a sufficiently described set of sentences and an interpretation of these. To present Hirsch’s argument, I will have to follow this practice. Thus when I in the following talk of different languages, I will be talking about completely interpreted languages where the truth-conditions of sentences are already defined.

#### 1.2.4 The existential assumption

The fourth assumption connects Hirsch’s arguments to Quinean ontology. Hirsch assumes that ontological questions are answered by considering which quantificational sentences are true.<sup>21</sup> A quantificational sentence is a sentence having a term satisfying the core inferential role of the English quantifier. We say that such sentences have a “quantifier expression” or simply a “quantifier”. Most abstractly, a quantifier is an expression that satisfies the “formal-syntactic inferential roles familiar from formal logic” of the existential quantifier (Hirsch 2011, p. xiv). The only examples of such formal-syntactic inferential roles Hirsch suggests are existential introduction and existential elimination, so I will take the validity of these inference schemata to sufficiently define a “quantifier expression”. Another way of using the word “quantifier” is for the quantifier expression of a specific language. This is the expression in that language that plays the same inferential role in that language as the English quantifier does. Context will ensure that these notions are kept apart (see also sections 1.3 and 2.3).

Let this be the definition of a *quantificational sentence*: A quantificational sentence is a sentence of a language that has an expression that satisfies the formal-syntactic inferential role of the existential quantifier from formal logic. This sits well with what was said about Quinean ontology in the introduction. Ontological questions are answered by determining which quantificational sentences are true. If the paraphrased quantificational sentence  $\exists xFx$  in language  $L_i$  is true, then the ontological question of whether Fs exist is solved for speakers of  $L_i$ , and there is no further ontological question about Fs. The place where Hirsch differs from Quinean ontologists is in how one determines whether quantified sentences are true. While Hirsch

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<sup>21</sup> Hirsch does not formulate this assumption explicitly in writing, but his examples of ontological disputes always involve sentences starting with “there is” or “there exists something”, and he writes, “to know the truth-value of the sentence ‘There are tables’ is to know whether there are tables (Hirsch 2011, p. 101). In all of Hirsch’s writings, the question of relevance for ontology is whether sentences involving one of the existential idioms are true.



thinks this is solved solely by considerations of linguistic interpretations, mainstream ontologists answer such questions by the elaborate method described in the introduction.

### 1.3 Quantifier Variantism

#### 1.3.1 The position

In section 1.1, I quoted an important paper by Hirsch where he describes quantifier variantism as the thesis that the “the world can be correctly described using a variety of concepts of ‘the existence of something’” (2011, p. 68), and added to that characterization that none of these different concepts are metaphysically privileged. I chose that formulation because it nicely captures the essence of the thesis. That formulation is from 2002, however, and Hirsch has developed his view since then. To arrive at the best and most recent formulation of quantifier variantism, I have chosen to operate with a formulation of the thesis from the introduction of Hirsch (2011) supplied with other papers on quantifier variantism.<sup>22</sup>

A succinct formulation of quantifier variantism takes it to consist of the following two claims (Hirsch 2011, p. xiv):

- (i) “There are a number of possible truth-conditionally equivalent ontological languages.”<sup>23</sup>
- (ii) “The possible truth-conditionally equivalent ontological languages are of equal metaphysical merit.”<sup>24</sup>

Call (i) weak quantifier variantism, and (ii) strong quantifier variantism. Weak quantifier variantism may be true without strong quantifier variantism being true.<sup>25</sup> It is clear throughout Hirsch’s work that he takes “quantifier variantism” to consist of both claims, but it is advantageous to consider them apart because the arguments for them differ.

An *ontological language* in (i) and (ii) is a language individuated by which quantificational sentences are true in that language, and two languages are different ontological lan-

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<sup>22</sup> As a matter of fact, the introduction in Hirsch (2011) seems to characterize quantifier variantism in at least two distinct ways (in one way on p. xii and another on p. xiv). I have tried to find the best formulation of the thesis to avoid attacking a straw man, and the definition in Hirsch (2011, p. xiv) seems to me the clearest.

<sup>23</sup> This corresponds to the claim of “several concepts of ‘the existence of something’” from the introduction. As will be readily explained, these different languages will have different quantifiers, and as Hirsch uses the terms, a “concept of ‘the existence of something’” just is a quantifier.

<sup>24</sup> This corresponds to the claim that there is no ontological language that is metaphysically privileged.

<sup>25</sup> Can (ii) be true without (i) being true? Yes, because if there is only one ontological language, then all possible truth-conditionally equivalent languages are of equal metaphysical merit. This vacuous truth of (ii) will not be important, however, because I consider such cases (whether there is only one ontological language) as denials of (i), and there seems to be no interest in (ii) if (i) is false.

guages if they provide different answers to ontological questions. So if  $\exists xFx$  is true in  $L_1$ , and  $\sim\exists xFx$  is true in  $L_2$ ,  $L_1$  and  $L_2$  are different ontological languages.

Recalling Hirsch's intensional view on language, two languages are *truth-conditionally equivalent* if, "for any sentence in one, there is a truth-conditionally equivalent sentence in the other", and two sentences are truth-conditionally equivalent if, "relative to any (actual or possible) context of utterance, they are true with respect to the same possible worlds" (Hirsch 2011, p. xi–xii). So for any proposition expressible in one language in any context, the same proposition is expressible in the other language in the same context. Example: Assume that the sentences "There are tables" and "There are particles arranged tablewise" belong to different languages, and that both sentences hold true in the same set of possible worlds.<sup>26</sup> In that case, the sentences are truth-conditionally equivalent, and if one could continue the procedure for every sentence in each language, the languages would be truth-conditionally equivalent, but different ontological languages. It follows that the languages will have the same fact stating, or expressive, power (see Hirsch 2008b; Eklund 2007, 2009).

(ii) says that truth-conditional equivalence is sufficient for the languages to describe the world equally well, and that there are no further constraints on which language one *should* speak when talking about what there is.<sup>27</sup> If the languages can express the same content, then one cannot be any better metaphysically than the other. So even though two different ontological languages, e.g.  $L_1$  and  $L_2$ , are different with respect to what they say there is, e.g.  $Fs$ , they describe the world equally well as long as they can express the same intensions. For quantifier variantism to be true, then, there have to be (i) a number of different ontological languages that are truth-conditionally equivalent, and (ii) these languages are of equal metaphysical merit.<sup>28</sup> Thus "metaphysical merit" is reduced to truth-conditional equivalence. This reveals the importance of the intensional assumption; see Hawthorne (2009) for criticism.

A common characterizations of quantifier variantism is that there are several meanings of the concept of "the existence of something" or several candidate-meanings for the quantifi-

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<sup>26</sup> I here ignore the question of whether tables are ontologically multiply realizable, as does Hirsch.

<sup>27</sup> This holds only as long as the languages are not expressively impoverished relative to a third language. If there are facts that can only be expressed in a third language, and not in the other two, also Hirsch would claim that one should use that language to describe the world; one may make more true claims about the world. Crucially, Hirsch thinks that differences in which physical objects languages say exist don't have any bearing on the expressiveness of languages; languages can differ ontologically and still be truth-conditionally equivalent.

<sup>28</sup> If two languages are truth-conditionally equivalent in this sense, we have a translation between them. We can define a translation function between the two languages, such that a sentence in one is mapped to the sentence in the other language that expresses the same character. If a sentence is be mapped to a sentence with another quantificational structure, then the languages have different quantifiers. For instance, if the quantifier of one language binds a singular variable in a predicate, and the sentence in the other language it is mapped to involve plural quantification, then the languages have different quantifiers. Thus if the sentences "There are tables" and "There are particles arranged tablewise" hold true with respect to the same possible worlds, they will be mapped to each other. As a consequence of this, ontological questions will receive different answers in these languages.

er.<sup>29</sup> This is a correct description if we grant Hirsch the following: The meaning of the quantifier (over and above its inferential role) is defined by how it interacts with other terms of the language to determine the truth-conditions of sentences (Hirsch 2011, p. 71). Thus  $\exists x$  would have different meanings in  $L_1$  and  $L_2$ , because  $\exists xFx$  is true in  $L_1$ , and  $\sim\exists xFx$  is true in  $L_2$ . Note straight away that quantifier variantism is not the thesis that *only* the quantifier has different meaning in alternative ontological languages. It entails that the truth-conditions of singular sentences varies from one ontological language to another (Hirsch 2011, p. xii).<sup>30</sup>

As noted, Hirsch must define the meaning of a quantifier as something over and above its conceptual role. What may this be? Hirsch answers:

In general, we explain the meaning of a logical constant by describing the role it plays in determining the truth-conditions of sentences. ... [W]e explain the relevant change in the meaning of the quantifier, which will render the mereologist's sentence true, roughly as follows: In the new meaning, any sentence of the form "There exists something composed of the *F*-thing and the *G*-thing" is true if the expression "the *F*-thing" refers to something and the expression "the *G*-thing" refers to something. (2011, p. 71–72)

Thus what's over and above the conceptual role is the quantifier's contribution to the truth-conditions of sentences. The sentence "There is something composed of the Eiffel Tower and Plato's nose" is false in English, but true in the artificial language *Universalese*, which is defined to make the typical assertions of universalists, i.e. philosophers who believe that for any plurality of objects, there is something they compose, come out true. If we assume that these languages are truth-conditionally equivalent (see below), there must be a difference in the meaning of the terms. One of the expressions that contribute to this meaning-difference is "There is something", which thus plausibly has different meanings in these languages. The alternative would be to "blame it all" on the predicates and names, but given that a consequence of the truth of this sentence is that an object with the Eiffel Tower and Plato's nose as parts is in the universal domain of *Universalese*, and thus can be quantified over, it is plausible to say that also "There is something" has another meaning in *Universalese* than it does in English.

We can give another argument for the claim that the quantifier has different meanings in the languages. For at least in the case of someone who believes that two things never com-

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<sup>29</sup> Thus the suggestion is not the trivial claim that the strings of letters "There is" or "exists" can be assigned different meanings because language is conventional, nor that obviously false claim that in our sense of the quantifier there both are and aren't tables. It is the claim that the quantifier shares inferential properties, but differs in meaning.

<sup>30</sup> There is much more to be said here, but the in-depth discussion is projected to chapter 2. There I discuss the notion of "truth-conditional equivalence" in more detail, and I also discuss an objection based on an argument by Harris (1982) and Williamson (1987–1988) to the effect that if two expressions of different languages satisfies the same formal-syntactic inferential role, they must have the same meaning. If this argument is sound, it entails that there can be no quantifier variance.

pose something (a nihilist), and a universalist, they will disagree about what there is in a vacuum chamber containing two mereological simples (things without proper parts). The universalist will accept, and the nihilist will reject, the sentence:<sup>31</sup>

$$\exists x \exists y \exists z (x \neq y \ \& \ x \neq z \ \& \ y \neq z)$$

This sentence is construed to contain only quantifiers, truth-functional connectives, and the identity predicate. If we suppose, with Hirsch, that the disputants speak different truth-conditionally equivalent ontological languages, then because there is no equivocation in the meaning of the truth-functional connectives or the identity predicate, the universalist and the nihilist must be disagreeing on the meaning of the quantifier (Sider 2009, p. 390).

If what I have said is correct, a consequence is this. If two languages are truth-conditionally equivalent, yet quantify over different things: they have different quantifiers (in the required sense). But then it follows that if two truth-conditionally equivalent languages have different ontologies, they have different quantifiers, for the ontology is simply what one quantifies over. This fits well with how Hirsch formulates his view, but is a consequence he doesn't point out directly.

It might seem counterintuitive that the things in the domain of language contribute to individuate quantifiers. But we must recall that we are now only talking about the sense in which different ontological languages have “different quantifiers”, and this sense is naturally linked to the domain. If “quantifier” is understood as conceptual role, then the “meaning of the quantifier” is independent of the domain.<sup>32</sup>

In Hirsch's terminology, the denial of quantifier variantism is quantifier *invariantism* (Hirsch 2011, p. xiv). There are two ways of being a quantifier invariantist: either by denying (i) or by denying (ii). Call those who reject (i) *quantifier invariantists*.<sup>33</sup> They hold that there are not several truth-conditionally equivalent ontological languages, there is only one. This position is the topic of chapter 2. The other option is to accept (i), but deny (ii) and be a *normative*<sup>34</sup> *quantifier invariantist* (Hirsch 2011, p. xv). Normative invariantism holds that even though there are several truth-conditionally equivalent languages, there is a best, privileged

<sup>31</sup> This argument is from Sider (2009, p. 390). Sider (2009, p. 387–390) provides arguments for why one cannot blame only the predicate for the meaning-change involved in quantifier variance.

<sup>32</sup> We can note that the interpretative assumption, i.e. the claim that interpretation is done at the level of sentences, plays a role here. The meaning of the quantifier is construed not as a function of what is in the domain independently of which sentences are true, but instead as a function of which quantificational sentences are considered true in a given language. Because of the language assumption, we can define different languages where stipulated sentences are true, and as long as we can show that such languages are truth-conditionally equivalent to e.g. English, we have different ontological languages, and thus different answers to ontological questions.

<sup>33</sup> Hirsch calls this position “necessity” quantifier invariantism (2011, p. xv), because it is then necessary to use the one quantifier. I've chosen to omit “necessity” as I don't think it contributes much to the term.

<sup>34</sup> We call it normative invariantism because it says there is a *best* language for talking about what there is.

language to talk about what there is. Because the languages are truth-conditionally equivalent, the normative invariantist must hold that there is a further epistemic virtue that makes it rationally required to describe the world in a specific language: truth is not enough when doing metaphysics. I take this to be the position of Sider (2011), and this position is discussed in chapter 4.

We can now clarify why quantifier variantism plausibly is a local thesis: It requires *argument* to show that there are two different ontological truth-conditionally equivalent languages in a given ontological dispute. Quantifier variantism would prove false for a dispute over certain objects if two languages where the different views came out correct were not truth-conditionally equivalent. Hirsch thinks that in the dispute between nominalists and platonists there is no language which doesn't quantify over numbers that is truth-conditionally equivalent to a language that does quantify over numbers. Thus Hirsch believes that there is no quantifier variance in disputes about mathematical objects (Hirsch 2011, p. 243–245).

A last topic that must be mentioned is the relationship between quantifier variantism and realism. A common criticism of Hirsch (and especially his predecessor Putnam) is that quantifier variantism leads to an unattractive anti-realism. Hirsch insists that this is not the case, though there are versions of “realism” incompatible with quantifier variantism (2011, p. 79). I'm inclined to agree with him on this; quantifier variantism doesn't entail anti-realism. In any case, this will not be a central criticism of the present discussion (though see section 4.5)

We may now formulate a refined version of the deflationary argument based on quantifier variantism that I alluded to in section 1.1:

- P1. There are several truth-conditionally equivalent ontological languages (weak quantifier variantism)
- P2. If several ontological languages are truth-conditionally equivalent, then they are of equal metaphysical merit (strong quantifier variantism)
- ∴ C. The truth-conditionally equivalent ontological languages are of equal metaphysical merit.

The argument is valid, and the premises are the claims of quantifier variantism. We may call this the argument from arbitrary languages, as it trades on the apparent arbitrariness of ontology when we appreciate the fact that ontological questions have different answers in the different ontological languages. As I will presently exemplify, Hirsch argues that ontological languages reflecting popular positions in the ontology of physical objects all are truth-conditionally equivalent. Noticing that there are several ontological languages, and none of them privileged, one may worry why we should care about the truth in *our* language, or in any other language, for that matter. For they are all of equal metaphysical merit. Ontology be-

comes the provincial activity of determining which language one is speaking. When one has done that, the ontology follows. We will come back to this argument, but it attracts attention to a potential deflationary consequence of the truth of quantifier variantism.

### 1.3.2 Arguments for quantifier variantism

So much for background theory and abstract characterizations. I will give two examples of how Hirsch argues for quantifier variantism for a given dispute in the ontology of physical objects. The general form of the arguments is as follows:

1. Start with two different ontological languages,  $x$  and  $y$ .
2. Then show that:
  - a. If we assume  $x$ , then for any sentence in  $y$ , one can find a truth-conditionally equivalent sentence in  $x$ .
  - b. If we assume  $y$ , then for any sentence in  $x$ , one can find a truth-conditionally equivalent sentence in  $y$ .

In both arguments, Hirsch takes well-known ontological positions in the ontology of physical objects and stipulates that there are distinct languages in which sentences corresponding to the characteristic assertions by adherents of these theories come out true. Quantifier variantism requires merely that there are several *possible* ontological languages, so this stipulation might be considered a possibility-statement of such languages.<sup>35</sup> I will therefore speak about ontological languages such as Endurance- and Perdurant-English, Universalese and Nihilish. These names are shorthand for languages in which typical utterances by endurantists, universalists and so on are *true*. Thus they are consciously construed to have specific truth-conditions. For instance, in a language construed to make universalism come out true, the linguistic string “There is something having the Eiffel Tower and Capitol Hill as parts” is true if what we would usually refer to as the Eiffel Tower and Capitol Hill exists. In this sense, the languages are completely interpreted. This is in contrast to English, where we don’t really know whether this sentence is true or false (at least not without argument; section 1.4).

Recall that different ontological languages are thought to be able to express the same propositions. So the reason why assertions by speakers of these languages come out true even though they may seem to contradict each other is that they express the same propositions.

The general conclusion of quantifier variantism is supposed to be that certain ontological disputes *per se* are futile. The strategy described by (1)–(2) applies to only some positions of a given dispute, so does not warrant a *general* deflationary conclusion. To establish deflationism firmly, one will have to repeat the exercise for all the different answers to the ontological

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<sup>35</sup> That there are several ontological languages follows from the interpretative and language assumptions. If a language merely is a set of sentences associated with characters, we may consider the surface structure of sentences apparently expressing different ontological positions and yet associate them with the same characters.

question. That being said, I will only go through two alternative positions for each dispute below for practical reasons, but if the argumentative strategy is successful, I suppose that it has general relevance.

Step (1) is relatively unproblematic if we assume Hirsch's view on language. As we will see, the way Hirsch establishes (2) is by suggesting methods for construing the truth-conditions of an alternative ontological language in another ontological language, and vice versa. This method will be general, characterized by example or recursion, and is intended to persuade the reader that the process could be carried out for all sentences.

I will illustrate the argumentative strategy described by (1)–(2) by going through two of Hirsch's favorite examples; the dispute over the persistence of objects and the dispute over when some objects compose a further object.

***Existence over time: perdurance vs. endurance***<sup>36</sup>

In slogan form, endurantists believe that objects persist by being “wholly present” throughout their lifespan, and perdurantists believe that objects persist by having temporal parts existing at different times (Lewis 1986; Sider 2001b). Assume that the dispute between perdurantists and endurantists only concerns the existence of temporal parts, as Hirsch does (2011, p. 222). Following the above schema, the argument is that (a) an endurantist can adopt a “perdurantist language”, and (b) a perdurantist may adopt an “endurantist language”, and this adoption of a different ontological language shows that the newly adopted one is truth-conditionally equivalent to the original one. We may note that strictly speaking, this is the inverse of (b). The essence of the argument is nonetheless the same: one shows that two languages are truth-conditionally equivalent by providing a way of associating sentences of one language with a truth-conditionally equivalent sentence of the other. How one does this is irrelevant.<sup>37</sup>

Suppose that Edna is an endurantist and talks like one, but she wishes to pass herself off as a perdurantist. Call the language she originally speaks E-English, in which she may truthfully utter sentences like “Lincoln was bearded in 1860”. Hirsch claims that Edna may adopt the following convention to pass herself off as a perdurantist: “Henceforth I will use the expression ‘temporal part of an object’ when I want to talk about how an object is at a certain time. I’ll say ‘Lincoln had in 1860 a temporal part that was bearded’ to describe the situation in which Lincoln was bearded in 1860. In general, I’ll use a sentence of the form ‘*a* has at time *t* a

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<sup>36</sup> I have stripped down Hirsch's original argument to make it clearer and hopefully more successful in showing what I take it is supposed to.

<sup>37</sup> I've chosen to follow Hirsch's way of presenting the argument. On the one hand, it would be a bit tidier to follow the schema of (ii), but on the other it would require divergence in presentation from Hirsch's original paper, and I consider the costs of reformulating Hirsch's argument to outweigh the benefits.

temporal part that is  $F$  to be true of any situation in which  $a$  is  $F$  at  $t$ " (Hirsch 2011, p. 223).<sup>38</sup> Call the language Edna speaks after adopting this convention P-English.

To establish (b), Hirsch asks us to consider a perdurantist, Pedro, who adopts a "secret language that will enable him to sound like an endurantist" (Hirsch 2011, p. 223). Thus he adopts the following convention: "Henceforth I will in every context restrict my quantifier to objects accepted by endurantists – roughly, objects other than (proper) temporal parts of ordinary objects" (Hirsch 2011, p. 223). Call the language Pedro speaks after consistently adopting this convention E-English.

Hirsch claims that in P-English, typical assertions involving the (proper) temporal parts of objects are true, but that they are false in E-English (Hirsch 2011, p. 223). How should one evaluate this example? First, one might worry about the plausibility of adopting "another language" by adopting a convention. Hirsch doesn't address this, but we may answer that it seems we can at least start uttering sentences belonging to another language as long as we know the truth-conditions, and thus flawlessly can carry out the language-shift. Eventually we may end up speaking the "new" language. Second, the argument assumes that Edna and Pedro were speaking different languages before adopting the conventions. But it is not clear that simply because they accepted different theories of persistence, they spoke different languages initially. To avoid that problem, we may suppose that Edna from infancy belonged to an endurantists community and that Pedro belonged to a perdurantist community (see Dorr 2005).

Given these assumptions, the function of the conventions is to clarify how Edna and Pedro may define the truth-conditions of different ontological languages in their own, prior, language. If they carry on following the conventions, they will systematically and successfully utter sentences corresponding to the sentences perdurantists and endurantists accept. So for instance, when Edna wants to talk about Lincoln being bearded in 1860, she talks about him having a temporal part. As a result, they know how to define the truth-conditions of the alternative ontological languages in their own prior language, which is just what step 2 requires.

Hirsch's central claim is that "When we ask whether objects have temporal parts it seems that our answer ought to be 'yes' if our language is P-English, and 'no' if our language is E-English" (Hirsch 2011, p. 223). In P-English, which is the language Edna speaks after adopting the convention, the sentence "Lincoln had in 1860 a temporal part that was bearded" is true. In E-English, this sentence (string of letters) is false, because there are no temporal parts (to quantify over with Pedro's quantifiers anyways). This conclusion doesn't obviously follow from the example, however. If the point is that if we already spoke P-English, then the existence of

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<sup>38</sup> Hirsch's (2011, p. 222) way of introducing these conventions is by having Edna (and Pedro) "write in their diaries" the directions of the conventions. I have avoided this aspect of the story, as it is inessential to the argument.



temporal parts simply follows from the stipulation that we already spoke P-English.<sup>39</sup> The problem is that it isn't obvious that when Edna uses sentences of P-English, her utterances are true, because they do not conform to her beliefs, and the same goes for Pedro's utterances in E-English. Reply: The urgent point is whether the conventions *explain* the truth-conditions of P-English sentences in E-English, and for E-English sentences in P-English, cf. (b). This is how Hirsch describes the relationship between E-English and P-English:

... there is a certain set of sentences and a certain set of characters, and the characters are distributed differently over the sentences of the two languages in such a manner that the sentences asserted by endurantists in one language have the same characters as the different sentences asserted by perdurantists in the other language. (Hirsch 2011, p. 234)

The example of Edna and Pedro is supposed to show that this is a correct description of the case. If it is, the languages are truth-conditionally equivalent, for Hirsch's description of the case entails that Edna has understood how to use P-English, and Pedro has understood how to use E-English. Another point can be made: When Edna speaks P-English, she is supposed to be confident that her P-English utterances are merely linguistic variations of the characters she expresses in her primary language. The two languages are truth-conditionally equivalent, but the existential assumption implies that the answers to ontological questions are different. This should elicit the feeling that something is wrong with ontology.

The deflationary conclusion Hirsch draws from this argument is that because P-English and E-English describe the world equally well – one may express exactly the same intensions using these languages, but use different sentences doing so – there is no point in arguing over whether there are temporal parts.<sup>40</sup> There is no point in trying to mimic the “quantificational structure” of the world, because the world can be described equally well by languages with different quantifiers.

### ***Composition: “common sense” vs. universalism***

A second dispute in which Hirsch claims there is quantifier variance is the dispute over the special composition question, i.e. “Under what circumstances do several things compose something?”<sup>41</sup> Hirsch (2002a) imagines a dispute between a mereologist,<sup>42</sup> who believes that compo-

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<sup>39</sup> This follows from the assumption inherent in the definition of the languages: the languages are defined to make the distinct ontologically sentences come out true. One may object that this definition of a languages is incomplete, presupposing the conclusion. However, it follows from the intensional, interpretative and existential assumptions.

<sup>40</sup> If anything, there is the question of exactly which language ordinary language users speak, which is central in the argument for linguistic choice.

<sup>41</sup> The question was first posed by Peter van Inwagen (1987). The question, and its implications, is thoroughly discussed in van Inwagen (1990). van Inwagen (1990) considers several formulations. The official formulation is “When is it true that ‘ $\exists y$  the  $x$ s compose  $y$ ?’” (van Inwagen 1990, p. 30). The question is intimately related to mereology, the theory of part-whole-relations, but we will not focus on this aspect of the debate, because it is not directly relevant to the discussion of Hirsch's arguments.

sition always occurs; for any things whatsoever, there is something they compose,<sup>43</sup> and an “anti-mereologist”, who denies this, and who believes in the composite objects ordinary English speakers normally seem to quantify over. Again, the strategy is to show that for any sentence in one language, one can find a truth-conditionally equivalent sentence in the other.

Consider a sentence that mereologist will be disposed to assert: “There exists something that is composed of Clinton’s nose and the Eiffel Tower”. According to Hirsch, this sentence “can be interpreted in a way that makes the sentence true or in a way that makes the sentence false” (2011, p. 69). Because “both interpretations are available to us, we have a choice between operating with a concept of “the existence of something” that satisfies the mereologist or operating with a different concept that satisfies the anti-mereologist” (2011, p. 69). When Hirsch writes that “both interpretations are available to us”, he supposedly mean that we can understand the truth-conditions of the two possible languages in which the sentence is respectively true and false, and thus understand their “concept of existence”, i.e. the different quantifiers of two truth-conditionally equivalent languages.

Hirsch gives the following suggestion for truth-conditions for the mereologist interpretation of the sentence if one speaks the anti-mereologist’s language: The sentence “There exists something composed of the F-thing and the G-thing” in the mereologist’s language is true if the expressions “the F-thing” refers to something and the expression “the G-thing” refers to something” in the anti-mereologist’s language (Hirsch 2011, p. 72). He calls this “explaining the relevant change in the meaning of the quantifier” (Hirsch 2011, p. 72).<sup>44</sup> We here have a procedure for finding the truth-condition of an arbitrary sentence of Universalese in the language of the anti-mereologist, and thus one side of step 2 is accomplished.

Hirsch (2002a) does not provide a translation-scheme for the mereologist to explain the truth-conditions of the anti-mereologist’s language. Perhaps he didn’t consider it important, or perhaps he thought it was obvious. To remedy this, we may first note that it will be difficult to provide a principled definition of which anti-mereologist sentences are true in the language of the mereologist. The anti-mereologist utters sentences about composite objects typical of those of English speakers, and as far as we know, there is no well-defined criterion by which

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<sup>42</sup> This position is usually called “universalism”. I will follow Hirsch’s definition for expository reasons.

<sup>43</sup> I am trying to keep the discussion as non-technical as possible, but this position, universalism, is often characterized by the acceptance of the ‘theorem of unrestricted composition’, which is a theorem of ‘classical’ mereology as formulated by Leonard and Goodman (1940), and states that for all things (quantifying plurally), there is something (singular quantification) such that those things compose that thing.

<sup>44</sup> A superficial problem is that Hirsch hasn’t provided a general explanation of the truth-conditions of sentences in the universalist’s language. The sentence “There exists something composed of a F-thing+G-thing and a F’-thing+G’-thing”, where the predicates are predicates of English and “+” the mereological sum-function, makes sense for the universalist, but Hirsch’s translation scheme won’t give it truth conditions. The problem is merely that Hirsch hasn’t given a recursive definition of the truth-conditions of Universalese in English, but that is quite easily done.

English speakers decide which pluralities compose a thing, and thus are quantified over by a singular quantifier. This has been thoroughly illuminated in the literature (Cartwright 1975; Lewis 1986, p. 213–214; Hirsch 1982; for an attempt at identifying a unifying theme, see Schaffer and Rose *draft*). We would thus lack a way of clearly identifying the limited subset of all the objects the mereologist talks about. We can nonetheless circumvent this problem by adopting the suggestion of Hirsch (2009) with respect to Pedro, and simply make the mereologist restrict her quantifiers to the mereological sums English speakers talk about.<sup>45</sup> Thus it seems that we have the kind of truth-conditional equivalence necessary for Hirsch’s “demonstration” of quantifier variantism for this dispute.

If this is correct, we have another example of a debate in which the two sides can explain the truth-conditions of the other party’s language, and thus have two truth-conditionally equivalent different ontological languages. The disputants are merely expressing the same truth-conditions using sentences with different quantificational surface structures.

### 1.3.3 Concluding remarks

We have seen that Hirsch’s examples illustrate how one may argue that two different ontological languages are truth-conditionally equivalent. The central element is whether a speaker of one ontological language may explain, or define, the truth-conditions of the other language’s sentences in such a way that she can always find a sentence in her own language that holds true with respect to the same possible worlds. If she can, then there is no difference in the expressive powers of the languages if they are individuated intensionally. This is not a general argument for quantifier variantism, however. It says nothing about why normative quantifier invariantism is false. The conclusion of truth-conditional equivalence has no bearing on whether truth is enough for metaphysics, but Hirsch seems to derive this from the intensional assumption. These questions are touched upon in chapter 4, but are of minor relevance to the truth of weak quantifier variantism and the argument from charity.

## 1.4 Linguistic Choice

### 1.4.1 The position

What I have called the argument from charity has taken various forms over the years.<sup>46</sup> I will formulate it by considering a “dilemma” which incorporates both variants of the argument.

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<sup>45</sup> This method presupposes that the mereologist knows which things English speakers commonly quantify over, or at least has access to English speakers to discover when they use singular quantification.

<sup>46</sup> In an early paper where it appears (Hirsch 2002b), the argument from charity is targeted at revisionary ontologists, i.e. ontologists who holds that many common sense judgments about what there is are a priori necessarily false (2011, p. 101). In that paper, Hirsch argues that the principle of charity implies that the common sense judgments are false in English. This is quite akin to the first horn of the dilemma. In other papers, Hirsch (2005, 2009) argues that philosophers with different ontological stands should, because of “correct linguistic interpretation”, be

Hirsch poses the following dilemma to philosophers engaged in an ontological dispute: “Either you are both speaking plain English, in which case you are both asserting trivial falsehoods, or each of you is in effect asserting trivial truths in your own ontological language, in which case your dispute is verbal” (2011, p. xiii). Hirsch says he has no stake in pressing either horn of the dilemma, but holds that these are the only two options (2011, p. xiii). Analyzing the dilemma a little further, it is construed to be open on whether correct linguistic interpretation implies that ontologists are speaking English or distinct ontological languages. Hirsch (2005, 2008a, 2009) seems to be convinced that correct linguistic interpretation of actual ontologists leads to the result that they are speaking distinct languages, but if this is incorrect, they are at any rate asserting “trivial falsehoods” in English when they state their non-commonsensical conclusions. Thus there would seem to be no adequate “language of ontology”.

The argument from charity is the argument that proves that one of the disjuncts of the dilemma is true. Thus it must show that either of (iii) or (iv) is true:

- (iii) If ontological disputants are speaking plain English, then ontological claims diverging from common sense judgments about what there is are trivially false.
- (iv) If ontological disputants are not speaking plain English, then they are speaking distinct ontological languages, asserting trivial truths in their own ontological languages. In this case, the dispute is verbal.

Hirsch (2002b) argues for (iii), but Hirsch has spilled most ink arguing for (iv) (2005, 2008a, 2009). When Hirsch says he has no stake in pushing either horn of the dilemma, he presumably means that because the antecedents of the conditionals in the dilemma contradict each other, and he believes both conditionals, there is really nothing to argue about here. It is the same view on linguistic interpretation that motivates both conditionals, and thus the next section elaborates Hirsch’s view on the principle of charity, which is central to the view on interpretation. Section 1.4.3 shows how the argument for (iii) and (iv) goes.

#### **1.4.2 The principle of charity**

It is uncontroversial that the principle of charity is central to linguistic interpretation. There are several formulations of the principle, but a least common multiple is that it demands of us that, when interpreting someone, we attempt to make as many as possible of her assertions come out true, or at least reasonable (Hirsch 2011, p. 148). If we have a choice between two interpretations of a set of sentences asserted by a speaker, and one interpretation would imply

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taken to speak different ontological languages, thus having a verbal dispute. My hope is that by using the dilemma in the exposition of the argument, I can capture both these ideas.

that the speaker is correct and rational in asserting those sentences, and the other implies that she is incorrect and irrational, then the principle of charity imposes a presumption in favor of the first interpretation (Hirsch 2011, p. 99). In essence, there is a presumption that widely accepted sentences are true or reasonable (Hirsch 2011, p. 150). If speakers consider a sentence as obviously true, such as “There is at least one building in New York City”, then that counts heavily in favor of interpreting it as true – so it is true. Given the existential assumption, there are buildings. Hirsch has a thoughtful and interesting discussion of the considerations relevant in linguistic interpretation (2003, 2005 and 2009). Besides considerations of “use” in general, Hirsch gives special attention to three elements of the principle of charity that he considers especially relevant when interpreting the participants of ontological disputes.

First, perceptual report sentences are accorded special weight in interpretation, what Hirsch calls “charity to perception” (2011, p. 149f). Hirsch assumes that perceptual reports are generally accurate (Hirsch 2011, p. 149). Given the importance of perceptual input in rational behavior, we should expect speakers to be correct about what they perceive. Perceptual reports demand more charity than most other assertions, because it is harder to explain why someone should be wrong about what they perceive. Ordinary people consider the sentence “There is at least one building in New York City” true based on their perceptual experiences, and there is therefore strong pressure to interpret it as true. Hirsch claims that it is highly implausible that we are wrong about things that are “right in front of our eyes” (Hirsch 2011, p. 90).

Second, we should not expect speakers to be wrong about what is *a priori* knowable. Hirsch calls this “charity to understanding” (Hirsch 2011, p. 149). Hirsch points out that we should assume fluent speakers of a language to have a “sufficiently adequate grasp of their linguistic and conceptual resources so that they don’t generally make *a priori* (conceptually) false assertions” (Hirsch 2011, p. 149). This is especially so if the *a priori* truths don’t require elaborate computation or calculations. For instance, the nihilist position implies that ordinary people are wrong about there being buildings in New York City. But given the assumption of section 1.2.1, the answer to the special composition question is knowable *a priori*, so ordinary people are wrong about an *a priori* matter. But by the nature of the *a priori* and the apparently trivial question of whether there are buildings in New York City, it is highly implausible that speakers are wrong about this, because ordinary speakers take it to be rather obvious that there are buildings in New York City. An interpretation that implies widespread *a priori* mistakes to speakers has little credibility, and should, if possible, be dispensed with on behalf of an interpretation that does not.

Third, Hirsch attributes special weight to “charity to retraction” (Hirsch 2011, p. 151f). Hirsch writes that “Certainly we must, other things being equal, favor an interpretation that

makes the community's retractions in the face of additional evidence come out right" (Hirsch 2011, p. 152). This is an element of interpretation Hirsch notes may single-handedly deliver the conclusion that a dispute is substantive: "Many disputes ... are immediately shown to be substantive by the consideration of charity to retraction" (Hirsch 2011, p. 152). For instance: If something that was assumed to be evidence for a judgment really is not evidence, one should consider revising one's earlier judgment. One should also consider revising one's judgment if it turns out that there is more relevant evidence than one assumed when making the judgment. We must not only try to maximize the number of assertions of a speaker that come out true, or give weight to perceptual reports and *a priori* assertions, but also consider whether it is plausible that the person is making a mistake and will retract her judgment later.

*Prima facie*, these elements, or refinements, of the general principle of charity are plausible. The essence of the principle of charity is that one should interpret speakers in such a way that they, everything considered, come out as rational (or "sensible") as possible (Hirsch 2011, p. 230). On the face of it, it is implausible that speakers should be wrong about what they perceive, or about *a priori* knowable truths (excluding mathematics and logic). Charity to retraction turns on the rational imperative of evaluating the relevant evidence (Hirsch 2011, p. 231). When it comes to the soundness of the argument from charity, what matters is whether the principle of charity, *when applied to ontological disputes*, leads to the truth of (iii) and (iv).

### 1.4.3 The argument from charity

As stated, Hirsch thinks that considerations of linguistic charity implies that either of the following disjuncts of the dilemma are true:

- (iii) If ontological disputants are speaking plain English, then ontological claims diverging from common sense judgments about what there is are trivially false.
- (iv) If ontological disputants are not speaking plain English, then they are speaking distinct ontological languages, asserting trivial truths in their own ontological languages. In this case, the dispute is verbal.

As noted, the truth of the consequents is supposed to follow from considerations of charity.

Consider (iii) first. The truth of the consequent of (iii) requires that a correct interpretation of English implies that non-commonsensical ontological claims are trivially false in English. As Ted Sider expresses it: "According to Eli Hirsch, non-commonsensical [English] ontological claims just couldn't be true" (2014, p. 565). This is because of strong metasemantic pressure to charitably interpret English sentences about ordinary objects as true, and as a consequence, revisionary ontological claims are false. The metasemantic pressure is generated

by the elements of charity just mentioned and the fact that English speakers seem to consider non-commonsensical existence claims to be false, and there is no countervailing metasemantic pressure (see Sider 2014, p. 565). I will only sketch Hirsch's reasons for believing this here, but they derive from what was said above. The argument is based on data points as the following:

- Ordinary English speakers consider the sentence "There is at least one building in New York City" true based on their perceptual experiences.<sup>47</sup>
- Ordinary English speakers consider the sentence "There is an object which has the Eiffel Tower and Barack Obama's nose as parts" to be obviously false.<sup>48</sup>
- Ordinary English speakers consider the sentence "Something in the yard is a highly visible brown wooden object that contains branches during the daytime and contains no branches during the nighttime" to be false.<sup>49</sup>

Hirsch thinks that these data points indicate a pattern: ordinary speakers of English will consider non-commonsensical ontological conclusions false. They will be so judged based on perceptual reports, and if the non-commonsensical ontological claims are true, then ordinary speakers are making widespread a priori mistakes. This is highly implausible. There are no metasemantic considerations that can outweigh those I have now mentioned, and thus correct linguistic interpretation implies that the sentences are true. So if revisionary ontologists claim to be speaking English, they are asserting falsehoods.

Now consider (iv). In Hirsch (2005), it is argued that correct linguistic interpretation of ontologists implies that they must be taken to assert truths in their own language, but then they are using different languages, so they are engaged in a verbal dispute. In the context of the dilemma, the significance of the argument is that if the disputants claim to *not* be speaking English (so reject (iii)), then considerations of charity imply that they are engaged in a verbal dispute. If they are engaged in a verbal dispute, mainstream ontology is misconceived.

Hirsch defines a (merely)<sup>50</sup> verbal dispute as 'a dispute in which, given the correct view of linguistic interpretation, each party will agree that the other party speaks the truth in his own language' (2009, p. 239). Consider an example with a universalist and a nihilist. Suppose

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<sup>47</sup> The nihilist rejects this sentence, and in general judges a big class of perceptual reports in English about composite objects as simply incorrect.

<sup>48</sup> The universalist believes in a very large number of objects that are unfamiliar to the ordinary English speaker. One can create a big class of examples of sentences the universalist would have to say are true, but that the ordinary English speaker apparently rejects.

<sup>49</sup> Given perdurance theory conjoined with universalism (a very common combination), one can create a big class of objects very unfamiliar to the ordinary English speaker, such as the object having a tree's trunk and branches as parts in the daytime, and only the trunk as a part during the night.

<sup>50</sup> Hirsch usually talks about "merely verbal disputes", but in the original text he here only uses the words "verbal dispute". As he seems to use the terms interchangeably, I will as well.

that we have a vacuum chamber that the nihilist and the universalist agree contains two simples. They will then produce seemingly contradictory utterances when talking about the number of objects in the chamber. The nihilist will say:

There are only two things in the chamber. (1)

The universalist, on the other hand, will utter:

There are not only two things in the chamber. (2)

This is because the universalist holds that there is a further thing in the chamber: a thing composed of the two simples.

In contrast to the apparently genuine disagreement between the nihilist and the universalist, Hirsch holds that we must understand them as involved in a merely verbal dispute. If the nihilist should, based on correct interpretation, take the universalist to produce a true utterance when uttering (2), but insist on (2) being false in her own language, and the universalist should take the nihilist to utter a truth when uttering (1), but insist that (1) is false in her own language, then they must express different propositions when uttering (1) and (2), and are not really disagreeing. Rather, they are speaking different ontological languages. This problem would generalize to other assertions about composite objects. Thus the crucial premise in the argument for (iv) is whether “correct interpretation” lead to the conclusion that both disputants should acknowledge that the other party is uttering the truth.

It is the principle of charity that is supposed to lead to this conclusion. First of all, we note that philosophers of different ontological views will give systematically conflicting perceptual reports. Faced with the Empire State Building, a universalist will be prone to utter “There is a tall building in front of me”, but the nihilist will reject this sentence. In general, ontological disputants will utter consistently different utterances in a wide range of situations. This use-consideration, in addition to the differences in perceptual reports and the fact that the questions are supposed to be *a priori*, should lead us to the following conclusion: According to correct linguistic interpretation of the disputants, they are speaking truthfully in their own language, and are engaged in a verbal dispute.

So far, charity to retraction has been ignored. Initially, one would expect that the philosophers, rational inquirers as they are, would retract their ontological positions – and thus the “languages” they are speaking – if faced with new evidence. Hirsch answer to this is the assumption that ontologists don’t retract their positions, rather they have reached a stage where “all is said and done”, and they rely on ontological axioms that they do not revise. If this



is correct, then charity to retraction won't have a bearing on how we should interpret ontologists (Hirsch 2011, pp. 159–161; 230–231).

Considering these elements of interpretation, then, we may conclude that if disputing ontologists are not speaking English (in which case their assertions would be false), they are speaking different ontological languages, and so engaged in a verbal dispute. Thus there is no point in engaging in contemporary ontology.

#### **1.4.4 Concluding remarks**

Hirsch argues that ontologists should agree that the correct linguistic interpretation of fellow ontologists implies that they are all speaking the truth in their own language, alternatively that they are simply wrong if they are speaking English. The argument for (iv) presupposes weak quantifier variantism.<sup>51</sup> If Hirsch's application of the principle of charity is correct, and there are no other essential elements of linguistic interpretation that must be respected, then questions of ontology will to some extent "have to do with issues of linguistic interpretation" (Hirsch 2011, p. 229). The sense in which ontological disputes are deflated is that one merely has to make a "linguistic choice" between analyzing ontological disputants as speaking English or having a verbal dispute, but in either case, ontology is futile. If ontological disputes are merely verbal, then there is nothing deep or substantive for philosophers to get hyper-theoretical about; nothing is substantively at stake beyond the correct use of language.

I've presented Hirsch's arguments in detail, and we can now supplement the roadmap of the introduction. In chapter 2, weak quantifier variantism is challenged. If it is false, then ontologists aren't engaged in a verbal dispute, but denying weak quantifier variantism will not in itself block the conclusion that non-commonsensical ontological claims are false in English. That question is discussed in chapter 3, where I first defend the notion that ontologists are speaking English, because Hirsch has misapplied the argument from charity, and then argue that revisionary ontological claims aren't "trivially false" in English, thus opening the door for the possibility of ontology in English. In chapter 4, I evaluate a suggestion to avoid the whole problem of several ontological languages by moving ontological disputes to a distinct, privileged ontological language (Sider 2011). If that is possible, the considerations of charity considered here would have no bearing on the possibility of mainstream ontological inquiry. On the other hand, such a suggestion is the most revisionary way of answering Hirsch, and should be considered last and as a last resort.

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<sup>51</sup> Hirsch (2011, 224) makes the "stipulation" that the truth-conditions expressible by the two languages are the same when he argues for linguistic choice.

## 2 QUANTIFIER INVARIANTISM

### 2.1 Overview

As it was characterized in chapter 1, weak quantifier variantism is the claim that there are a number of possible truth-conditionally equivalent ontological languages (Hirsch 2011, p. xiv). If this thesis is true, it seems to be arbitrary which ontological language we use to describe what there is, as the argument from arbitrary languages attempted to show (section 1.3.1). Take Edna and Pedro as examples. They were born to different linguistic communities, and quantifies over different things. But given strong quantifier variantism, neither of these languages is objectively better than the other – correctness of ontological position is simply to use one’s language correctly. The future of ontology looks bleak.

The rejection of weak quantifier variantism is quantifier invariantism. This is Hirsch’s description of the thesis:

... [T]here is only one (metaphysically) possible ontological language: it is impossible for any language to lack the quantifier-meaning that we have in our language. Linguistic constructions that purport to yield alternative ontological languages merely produce some form of secondary languages, but not possibly genuine (primary, natural) languages. (Hirsch 2011, p. xv)

Quantifier invariantism is the claim that there is only one quantifier meaning, and every language able to quantify employs the same quantifier. If all possible languages have the same quantifier meaning, then there is only one kind of ontological language in the sense of the definitions of chapter 1, the one having the quantifier meaning of (*inter alia*) English.<sup>52</sup>

If quantifier invariantism is true, then we have a partial defense of ontology against Hirsch. We can avoid the argument from arbitrary languages, for there is only one ontological language.<sup>53</sup> The argument from charity also partly depends on the truth of quantifier variantism, and if weak quantifier variantism is false, then any attempt to define an “ontologically privileged language” is unnecessary and pointless, for there is only one. Finally, weak quantifier variantism by itself has problematic consequences we would like to avoid (see below).

Most generally, quantifier invariantism is the position that something goes wrong when Hirsch argues that different ontological languages are truth-conditionally equivalent. This says nothing about *what* goes wrong: different arguments against weak quantifier variantism will lead to different analyses of exactly what’s wrong with Hirsch’s argument (there may of course be several problems). The unifying theme of this chapter is that the arguments discussed

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<sup>52</sup> This thesis has another advantage. It makes clear why ontology in different natural languages comes to the same.

<sup>53</sup> The argument is that we know that we speak different natural languages, so if we have good reason to believe that there aren’t different possible ontological languages, we should fall back on our ordinary languages.

herein focuses on ways of rejecting weak quantifier variantism, as opposed to arguing against the argument from charity (chapter 3) or strong quantifier variantism (chapter 4).

This chapter considers arguments against weak quantifier variantism (often just denoted “quantifier variantism”).<sup>54</sup> The first section discusses an argument by Peter van Inwagen that I find unsuccessful, but objecting to it helps us better understand quantifier variantism. Section 2.3 deals with two arguments that I think ultimately can be resisted, but the defense has problematic consequences. Section 2.4 argues that one version of quantifier variantism entails a problematic account of the semantics of mathematical terms, and section 2.5 argues that Hirsch’s notion of “truth-conditional equivalence” is difficult to grasp, and furthermore that Hirsch makes an unwarranted assumption. The last section argues that weak quantifier variantism has widespread consequences for how we conceive of ontological languages.

## 2.2 Peter van Inwagen’s incoherence argument

Peter van Inwagen has argued, directly and indirectly, against weak quantifier variantism. By “indirectly” I mean that van Inwagen has targeted Hillary Putnam’s “conceptual relativity,”<sup>55</sup> but those arguments would, if sound, create problems for weak quantifier variance as well. I will critically examine van Inwagen’s (2009) argument against Putnam’s (2004) conceptual relativity, but the focus is on whether the arguments defeat Hirsch’s flavor of ontological deflationism, not Putnam’s.

Putnam (2004, pp. 33–52) argues for a deflationism not unlike that of Hirsch.<sup>56</sup> The central claim is that we can extend the meaning of “there is” by convention. Putnam holds that in a situation like the vacuum chamber described in sections 1.3.1 and 1.4.2, one can either speak like a nihilist and say that there are only two objects, or extend the meaning, i.e. by convention adopt another usage, of “there is” so that it is true that there is a sum of the two simples, and hence three objects. Because we can choose which meaning of “there is” we wish to operate with, there is no deep question about what there is. This is very similar to Hirsch’s claim that “two interpretations are available to us” when describing which objects there are (2011, p. 69).

van Inwagen objects to this line of argument:

Extending the meaning of a term so that that term will apply to objects beyond those it already applies to is precisely analogous to extending a geographical boundary: you can extend a geographical boundary to encompass new territory only if that territory is already there. (van Inwagen 2009, p. 491)

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<sup>54</sup> The arguments against weak quantifier variantism are arguments against quantifier variantism as well.

<sup>55</sup> Putnam defends this idea in several works. In an early work (1981) it is called an “internalist view”, but by his (1987) the name of “conceptual relativity” has emerged. I will only discuss Putnam (2004) here.

<sup>56</sup> See especially Hirsch (1999) and (2002a) for a comparison of Hirsch’s view with Putnam’s.

van Inwagen's objection to Putnam and Hirsch is that extending the meaning of a term presupposes that there *already is something* to be in the extension of the new, extended term. When it comes to answering this objection, Hirsch's view has the advantage of having clarified some issues that gives him resources to reply to this argument.

First of all, Hirsch is not wedded to the notion that one "extends" one's quantifier.<sup>57</sup> "Extending" sounds like one is merely loosening a contextual quantifier domain restriction, but Hirsch is adamant that this is not what quantifier variantism amounts to (2011, p. 86f). van Inwagen's objection seems to attribute too much weight to the word "extending" the meaning of "there is". According to Hirsch, we change the quantifier by an ostensive definition of new truth-conditions for a language (Hirsch 2011, p. 80). The essence of language, thinks Hirsch, is merely the distribution of truth-conditions over sentences. This is done in one big sweep, altering the truth-conditions of the sentences in a systematic way, depending on the nature of the ostensive definition.

In the quoted passage above, van Inwagen says that extending the meaning of "there is" is like extending one's boundaries: we can do it only if the territory is already there. I take it that the point of this comparison is that for both existence and territory, "it's there or it isn't". If it's there, you can extend the meaning of "there is" to apply to it, but then it was already there, independent of the convention one adopted. If it isn't there, you can't "extend" the meaning of "there is" (or any other term) to apply to it: you would just end up speaking falsely. You cannot create objects by convention; that would be an unacceptable linguistic idealism.

van Inwagen's objection tries to show that the idea of extending the meaning of "there is" (in Hirsch's terms: the adoption of a new ontological language) is incoherent by appealing to the apparent truth of "it's there or it isn't". But the objection presupposes the quantifier of the language the objection is formulated in, and it's exactly the quantifier that is at stake. If one were confined to speaking the same language as van Inwagen does, then the objection would be sound – "it's there or it isn't", and either option is bad. However, when we change ontological languages, we also change the things we talk about – that's the point. We aren't constrained by the truth of "it's there or it isn't" in van Inwagen's language, for the meaning of "it" changes (because "it" can only successfully refer if there is something, and what there is depends on the quantifier), so the meaning of "it's there or it isn't" changes as well. van In-

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<sup>57</sup> I will not discuss whether Putnam is. I agree with the vast majority of commentators and critics of Putnam that his formulations of conceptual relativity too often borders on a kind of linguistic idealism or at least an unattractive anti-realism, perhaps susceptible to van Inwagen's arguments.

wagen's objection expresses a language-internal truth,<sup>58</sup> but when we adopt a new ontological language we "break free" of the two alternatives van Inwagen lays out for us by "it's there or it isn't". The language-internal truth that to extend a word's meaning presupposes that there is something there for the word to apply to, does not stand in the way for us to imagine new ontological languages with new quantifiers and evaluate whether they are truth-conditionally equivalent.

Above, I understood van Inwagen's objection to weak quantifier variantism as an argument for why one can't end up speaking other ontological languages because one can't extend the meaning of "there is". I took it to be a logical, or semantic, argument. But perhaps van Inwagen's objection instead has a robust realism as a premise, where the objects are "out there" like the territory is, and there is only one quantifier we can use to talk about those things – the English one. There are some problems with making this idea cogent, as it needs spelling out to explain how the English quantifier matches "the objects" perfectly, while the other suggested ontological languages does not, but I will here be content with noting that van Inwagen hasn't said anything in defense of such a realism, or filled it with content, so I will bypass this formulation of the objection. For a formulation and defense of such a robust realism, see chapter 4.

Putnam argues against the thesis that there is a singled "fixed in advance" meaning for "there is" (2004, 84). This thesis is quite similar to quantifier invariantism: there is only one sense of the quantifier.<sup>59</sup> van Inwagen argues that there *is* such a "fixed in advance" sense of the quantifier: "you need a fixed-in-advance sense of 'there is' to express your belief (a belief you must have if you are contemplating such a convention) that the class of 'new' things that the term is to apply to is not empty" (2009, p. 491). This again overlooks the radical shift in truth-conditions that occurs when we define an alternative language. Which sets there are is, in part, determined by which things there are. And which things there are is determined, in part, by which ontological language one is employing.<sup>60</sup> So it would seem we don't need a fixed-in-

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<sup>58</sup> By "language internal", I mean claims that hold within a language, but not between them. Given quantifier variantism and several possible ontological languages, some claims will only hold internal to a specific language. See section 2.3 and the (T)- and (T\*)-principles there for examples.

<sup>59</sup> A difference between Putnam and Hirsch here, which has only superficial impact on the general point, is that while Hirsch thinks there is one sense of the quantifier in English, Putnam thinks that there is a variety of uses of the quantifier in English itself, or alternatively that we can adopt a different use of the quantifier and still speak English, while Hirsch would consider that situation as a change of language (see Hirsch 2011, p. 80f). With regard to van Inwagen's argument, it is formulated against the claim that there are several senses of the quantifier in English, but it may be treated as a point against the "possible quantifier variance" of Hirsch as well, as I do here. At least construed as an objection to Hirsch, I think the argument fails.

<sup>60</sup> Of course not all sets depends for their existence on what physical objects there are. Also, ZFC is independent of which physical things there are, because one can take the empty set as the basic constituent and create sets with it as members, and so on.

advance sense of “there is” to successfully imagine new ontological languages that appear to be truth-conditionally equivalent to English or other ontological languages.<sup>61</sup>

A general insight of this answer on behalf of Hirsch is that we must acknowledge the breadth of the consequences of a language-shift. The next section expands on this theme, showing that the quantifier variantist is forced to accept some further consequences that perhaps aren’t that attractive.

### 2.3 The Eklund/Hawthorne argument

Matti Eklund (2007, 2009) and John Hawthorne (2006) have argued that weak quantifier variantism is false because different ontological languages aren’t truth-conditionally equivalent. Their argument depends on the standard Tarskian definition of truth for atomic sentences, so alternatively, the quantifier variantist must reject this Tarskian principle, which is a compelling principle of referential semantics. I shall argue that the quantifier variantist can meet the challenge posed by the Eklund/Hawthorne argument, but that doing so has unattractive consequences.

Eklund and Hawthorne’s argument goes as follows. Consider two individuals, Big and Small, who belong to different linguistic communities.<sup>62</sup> Big speaks a language, Biglish, in which speakers quantify over tables. Exhibiting the common linguistic capacity of name giving, he gives a certain table the name *a*. He can then truthfully assert “*a* is a table”. Small speaks a language in which one doesn’t quantify over tables, only over particles arranged tablewise, so a sentence of the form “*a* is a table” is false on any interpretation of *a* in Small’s language.

Suppose that weak quantifier variantism is true, and that this is a situation of two truth-conditionally equivalent ontological languages. It follows from Hirsch’s characterization of such cases that Small should consider Big’s sentence “*a* is a table” as *true* in Biglish, and vice versa (Hirsch 2011, p. 149; 229). Given quantifier variantism, they are both correct in their respective languages, but by our assumptions about Smallish, it is false in that language that there are tables. However, and this is Eklund and Hawthorne’s point, this implies that if Small uses Smallish to talk about Biglish (i.e. uses Smallish as a metalanguage), Small must reject that the following principle of Tarskian semantics holds for any target language, L:

- (T) For a sentence of the syntactic form “F(*a*)” of L to be true in L, the syntactically singular term “*a*” must refer.<sup>63</sup>

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<sup>61</sup> Whether we can end up *speaking* these languages is another matter, see section 3.2.

<sup>62</sup> The names of the characters are from Sider’s (2011, p. 181f) exposition of the argument.

<sup>63</sup> This is a modified version of the principle Hirsch (2011, p. 240) and Eklund (2009, p. 145) uses in their formulations of the argument. In spirit, it is similar to the premise used in Eklund (2007). Hawthorne applies a principle

If Small is a quantifier variantist, she must reject (T). This can be showed by a simple argument. If Small claims that Big's sentence "*a* is a table" is true in Biglish (which is a possible language), and accepts (T), then *a* refers. Smallish is the metalanguage in which we apply (T) to Biglish, so *a* must refer in Smallish. If *a* refers, then it refers to table, but by definition one cannot quantify over, thus can't refer to, tables in Small's language. So if we accept (T) and the described scenario, we must either accept the contradiction that one can and one can't quantify over tables in Smallish, or alternatively accept that Small's language is expressively impoverished relative to Biglish. In either case, weak quantifier variantism is false.

The only premise to challenge is (T), but on what grounds may the quantifier variantist do so? (T) is an important part of perhaps our best semantic paradigm, and has earned its keep.<sup>64</sup> If the only ground for rejecting (T) is that it implies the falsity of weak quantifier variantism, it would seem like a problematic *ad hoc* response from the quantifier variantist, providing pressure to reject quantifier variantism instead.

The quantifier variantist has a principled response, however.<sup>65</sup> She could say that different ontological languages have different names, predicates, and other expressions connected to quantification. To use Sider's phrasing, a name is "an expression that refers to a single thing; a predicate is an expression that applies to one or more things" and so on (2011, p. 181).<sup>66</sup> The tight connection between quantification on the one hand, and names and predicates on the other, is definitive of the semantic categories themselves. If names are expressions that refer to a single thing, and "thing" varies between ontological languages, then what it means to be a name varies as well. So different meanings of the quantifier would seem to lead to different senses of "name" and "predicate" in the different ontological languages. This in fact seems to be accepted by Hirsch (*inter alia* 2011, p. xii; 37; 157f), who notes that when we vary the meaning of the quantifier, we also vary the meaning of several semantic categories, such as names and predicates, even the meaning of "reference" (see below).

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akin to "Sentences of the form 'That is F' are true only if the speaker refers to something by 'that'" as holding of other speakers' languages (2006, p. 59).

<sup>64</sup> Eklund (2007) argues that if the quantifier variantist rejects (T), then there are semantics facts that she cannot express, and thus that a language in which (T) is false will be expressively impoverished. But it would seem that (T) rather is a part of a theory of semantics, and that other theories could express the same semantic facts using different vocabulary. Hirsch (2009) notes that perhaps (T) is a principle that will give you a nice semantic theory, but that such reasons are merely pragmatic, without purchase on the fundamental questions quantifier variantism tries to deal with. I join Sider (2011) in finding this response essentially correct.

<sup>65</sup> This reply on behalf of the quantifier variantist is not profound. Both Hirsch (2009) and Sider (2011, p. 181f) make quite similar points.

<sup>66</sup> This at least holds in first order logic. Perhaps this is too simple a criterion; Empty names don't refer, but arguably they are names. On the other hand, it would seem that other accounts of names will have the same consequences as those I spell out for this simple criterion, see Turner (2010, p. 15f).

What I've said requires that we use a semantic criterion for names and predicates.<sup>67</sup> But that would seem to be built into the quantifier variantist's treatment of names and predicates in a language anyway. The sense in which the quantifiers of different ontological languages differ is exactly semantically; by an inferential or syntactic criterion, different ontological languages have the same quantifier, but surely ontological languages are distinguished by their quantifier.

The suggested reply to the Eklund-Hawthorne-argument is then this: The quantifier variantist can say that (T), which says that "F(a)" being true in target language entails that *a* refers in the metalanguage,<sup>68</sup> does not hold when (T) is used to interpret a language with a different quantifier than the metalanguage. Different ontological languages will have different notions of name and predicate, which has consequences for whether it is true that *a* refers in Smallish. Big's expression "a" is not a name from the perspective of Smallish, because "a" doesn't refer to something<sub>Smallish</sub>, so there is little reason to accept the semantic paradigm (T) is a principle of (see below). Given this difference between ontological languages, the rejection of (T) is merely a matter of taking seriously the consequences of quantifier variance for semantics in general, and the semantic categories specifically. Thus it would seem that the quantifier variantist has a principled response to the argument, following naturally from the view itself.

A consequence of this is that the quantifier variantist must reject that any language can be a metalanguage for the purpose of providing a general referential semantics for all other languages. We should nonetheless note that the quantifier variantist can accept a weak interpretation of (T), on which it holds internal to a language, i.e. when the target and metalanguage is the same, but not when interpreting other ontological languages:

(T\*) For a sentence of the syntactic form "F(a)" to be true, the syntactically singular term "a" must refer.

(T\*) may be an important part of a language-internal semantic theory. Whether a semantic theory incorporating (T\*) is favorable depends on whether (T\*) gives a good account of the language one's speaking. That's a question largely independent of the question of quantifier variantism, but the potential truth of (T\*) in a certain language would not have any untoward consequences for the quantifier variantist.

In reply to what I've said on behalf of quantifier variantism, it can be objected that there is no reason to dispense with (T) if one uses an ontologically "more expansive" language.

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<sup>67</sup> As opposed to an inferential criterion, see Turner (2010, p. 14f).

<sup>68</sup> The truth-conditions suggested for "F(a)" in (T) follows from the standard Tarskian truth definition of atomic sentences (Hawthorne 2006).



If we use Biglish to interpret Smallish, then there is no pressure to reject (T), for Biglish can provide a Tarskian analysis of the truth-conditions of all the sentences of Smallish. This could be reason to prefer a “bigger language” in general for the purpose of providing a semantic theory, so Biglish is better than Smallish.

There are two responses to this. First, quantifier variantism can acknowledge that some languages are better than others for the purpose of providing a tidy theory of semantics. But the quantifier variantist can hold that this doesn’t have any ontological consequences; the languages are still truth-conditionally equivalent (see Hirsch 2011, p. 240f; Sider 2011, p. 182). The variantist might hold that this should be accepted by the mainstream metaphysician as well, as an argument from “better metalanguage” would itself seem to trivialize ontological disputes, privileging the most expansive ontological proposals – universalism, perdurantism, etc. – by default.<sup>69</sup> Second, the quantifier variantist can argue that the hope of a general complete theory of semantics across possible languages is anyway misconceived, not recognizing the consequences of quantifier variantism. Such a rejection of “bottom up”, or compositional, semantics, where the meaning of sentences is ultimately given by the meaning of its constituents, may be unattractive to some philosophers, cf. the interpretation assumption of section 1.2.3. Instead of pushing this objection, I’ll leave it to the reader to evaluate how problematic this consequence is.

Another argument, due to a proof by Harris (1982) and found in more accessible form in Williamson (1987-8), can be used to bolster the conclusion that the quantifier variantist should accept that the semantic categories connected to quantification differs between ontological languages. The point is that if we can have different existential quantifiers that nonetheless obey the same inference rules, then we can presumably create a language, *L*, which contains both these quantifiers.<sup>70</sup> But if they obey the same inference rules, then we can prove that the quantifiers are logically equivalent. To take an example from Sider (2007, p. 217): If one of the quantifiers is  $\exists_1$  and the other  $\exists_2$ , then if  $\exists_1\phi(x)$ , by  $\exists_1$ -elimination,  $\phi(a)$ . But then by  $\exists_2$ -introduction,  $\exists_2\phi(x)$ . As a consequence, the quantifiers range over the same things, and have the same meaning after all.

To avoid this conclusion, one must show that the just mentioned inference is invalid. One way of doing that is to adopt the suggested reply to the Eklund/Hawthorne-argument, and say that because different quantifiers entail different names and predicates, *L* is a disjunctive language. When constructing a language with two different quantifiers, one doesn’t have one stock of names and predicates. The inference-rules that connect a quantifier to names and

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<sup>69</sup> Indeed, both Eklund (2007, 2009) and Hawthorne (2006) argue for such expansive ontologies.

<sup>70</sup> This might in fact be of interest, because we are interested in knowing how the expressions interact.

predicates hold only between the quantifiers and predicates and so on of the same *type*. This is also the reply advocated by Sider (2007, p. 217f). Thus the above inference would be blocked because L operates with two types of quantifiers, and consequently two types of names and predicates. So if we have  $\exists_1 \phi(x)$ , then by  $\exists_1$ -elimination,  $\phi(a_1)$ , but it would be invalid to use  $\exists_2$ -introduction to get  $\exists_2 \phi(x)$ , because  $\exists_2$  and  $a_1$  are of different types.

A different reply would be to insist that one simply cannot construe a language like L, but without a further rationale, this would be a rather empty insistence. Expression meanings aren't like magnets repelling each other,<sup>71</sup> so the quantifier variantist must somehow have a principled rejection of inferences that entail equivalence. One such rejection is to hold that there is no inference from the formulae after  $\exists_1$  to  $\exists_2$  because names and predicates are inferentially connected to a distinct quantifier. This is the best reply I know of that the quantifier variantist can give, and it has the advantage of trading on the same ideas as the reply to the Eklund/Hawthorne-argument, thus avoiding further divergence from traditional logic.

I've argued that the quantifier variantist can respond to two potentially devastating semantic arguments against quantifier variantism. A consequence of this response is that the semantic categories of names and predicates themselves vary when we vary the quantifier. I also argued that this is a natural consequence of the view itself, because names and predicates are tightly connected to quantification. Thus for the different ontological languages  $L_1$  and  $L_2$ ,  $L_1$  will have names<sub>1</sub> and  $L_2$  will have names<sub>2</sub>;  $L_1$  will have predicates<sub>1</sub> and  $L_2$  will have predicates<sub>2</sub>. As the reply to the Harris-argument required, there are no completely general truth-preserving inferential connections from expressions of one of these languages to the other. It follows from this that predicates and names must have different meanings in the languages. Thus "red" in  $L_1$  is not the same predicate as "red" in  $L_2$ . Another route to this conclusion is to acknowledge that the treatment of predicates that sits best with Hirsch's intensional assumption is the orthodox understanding of them; the meaning of a predicate is given by the extension of the predicate, i.e. the set of things satisfying the predicate. The meaning of "red" is the set of red things, the meaning of "mammals" the set of mammals. So if we go from English to Smallish, "red" would get a new meaning, because the set of red things has changed (there is no such things as my red copy of *Metametaphysics*). So the meaning of predicates must vary.

We can now formulate a problem for quantifier variantism.<sup>72</sup> Suppose that two philosophers speak different ontological languages and that they disagree about whether there are tables or holes. By what I've said, they mean different things by "tables" and "holes". This has the consequence that believers in weak quantifier variantism cannot make the following

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<sup>71</sup> This phrase was used by Matti Eklund in correspondence.

<sup>72</sup> Thanks to Matti Eklund for helping me acknowledge this.

speech, attractive to some adherents of weak quantifier variantism: “While it may be absurd to deny that there’s any sense in which – for example – there is a hole in a perforated sock, it’s not absurd to deny that ‘in a metaphysical sense’ there exist holes” (Sider 2011, p. 172). For “hole” will have a one meaning in the language which employs the “metaphysical sense” of the quantifier, another in English. Similarly, if we accept the suggested reply to the two semantic arguments, then it is wrong to describe the dispute over persistence as one over the existence of temporal parts, as Hirsch does (2011, p. 222), for “temporal part” will have different meanings in the mouths of Edna and Pedro. Furthermore, it is misleading when Hirsch writes that the sentence “There exists something that is composed of Clinton’s nose and the Eiffel Tower” as being interpretable in one way that makes it true, and another which makes it false (2011, p. 69). For he seems to imply that the only part of the sentence that varies in meaning is the quantifier, but in light of the consequences that I’ve been pointing to follows from the reply to the Eklund/Hawthorne argument, also “compose” and “part” may have a different meanings.

In general, the suggested reply to the semantic arguments entail that the quantifier variantist cannot formulate her characteristic criticism of ontology. She cannot claim, as she usually does, that ontological disputes over *Fs* are deflated by quantifier variantism because in one sense of the quantifier there are *Fs*, and in another there aren’t *Fs*. For “*F*” doesn’t have the same meaning in these languages. It would seem that the question about the existence of *Fs* is left standing if we accept the suggested reply to the semantic arguments, for clearly, there not being *F\*s*, where *F\*s* aren’t *Fs*, doesn’t have a direct bearing on the existence of *Fs*. Thus there isn’t one sense in which there are *Fs*, and another in which there are no *Fs*. We can therefore formulate the following dilemma to the quantifier variantist with deflationary ambitions:

- (a) *Either* different ontological languages have different names and predicates, which means that the possibility of different ontological languages doesn’t have direct ontological consequences, *or*
- (b) The Eklund/Hawthorne and Harris arguments entail that quantifier variantism is false.<sup>73</sup>

The disjunct (b) presupposes that there are no other answers to the semantic arguments, but I don’t know of any other general reply to them, and as we saw, there are independent reasons to believe the consequence that names and predicates vary with the quantifier. If we therefore

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<sup>73</sup> As should be clear, the arguments and replies used to arrive at this dilemma are not entirely novel, but to my knowledge there is no formulation of this dilemma in the literature. Thus it is interesting to observe that Sider (2007, p. 218–219) apparently accepts both the reply to the Eklund/Hawthorne-argument, and at the same time seems to hold that the meaning of predicates doesn’t change (2011, p. 172).

try to avoid (b), we are pushed towards (a). If predicates and names have different meanings in different languages, there is no obvious way of rehabilitating the deflationary arguments based on quantifier variantism. This seems like a significant blow to quantifier variantism, for the main motivation for accepting the view was clearly that it would dispense with the difficult questions of ontology. That being said, quantifier variantism may still be true, but it has no impact on ontology; ontological disputes over *Fs* can continue independently of the truth of quantifier variantism.

## 2.4 The meaning of mathematical terms

I've argued that to avoid the semantic arguments, the quantifier variantist should accept that names and predicates also vary between ontological languages. As I also argued, accepting name and predicate variance has problematic consequences for the deflationary ambitions of quantifier variantism. The quantifier variantist might want to avoid these, so let us in the following suppose that the quantifier variantist has some other way of avoiding the semantic arguments than the one I suggested, and thus continues to hold that names and predicates in different ontological languages have the same meaning. This section argues that this view – that there are different ontological languages with different quantifiers but the same predicates – has another problem. Under plausible assumptions, this version of quantifier variantism entails that mathematical terms have different meaning in different ontological languages.

The argument starts with an observation made by Peter van Inwagen (2009, p. 483), inspired by Frege (1980): To say that *Fs* exist is to say that the number of *Fs* is not 0, and to say that *Fs* don't exist is to say that the number of *Fs* is 0. Assume that this is correct. Make also the plausible assumption that number-words have the same meaning in all languages; after all, mathematical objects, in contrast to e.g. physical objects, are “untouched” by the differences between ontological languages, and thus it's natural to expect that the meaning of the terms referring to mathematical objects is constant across ontological languages.<sup>74</sup> The contention is not that mathematical terms have the same meaning in different languages *because* the ontology is constant, but simply that it is a plausible assumption that they don't change meaning; even if we should accept variance in a term like “exists”, it is a radical move to posit variance in the meaning of terms like “0” or “1”.

Now consider a sentence that Hirsch believes is true in English: “Tables exist”. By van Inwagen's principle, this entails “The number of tables is not 0”. In contrast, Small truthfully asserts: “Tables don't exist”, and thus “The number of tables is 0”. Since the latter is just the

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<sup>74</sup> Thus the argument presupposes that there are mathematical objects. Hirsch himself presupposes platonism (2011, p. 191), but what about nominalists? Also nominalists need some theory about the semantics of mathematical terms, so I would think the argument has some relevance for them as well, but I cannot discuss this in any detail here.

negation of “The number of tables is not 0”, English and Smallish assign different truth-values to this sentence. Under the assumption that the languages are truth-conditionally equivalent, the meaning of these sentences must be different. It follows that either “the number of”, “0” or “not” means different things in English and Smallish. By the current assumption that predicates remain constant between ontological languages, “table” has the same meaning in both the languages. Since no one would want the logical operator “not” to vary in meaning, either “the number of” or “0” varies in meaning, and both alternatives go against the assumption that mathematical terms have the same meaning in different ontological languages.

I won’t be arguing for van Inwagen’s theory of the relationship between quantification and number. It is an intuitive and attractive claim, and to some extent a consequence of the notion of the extension of a predicate: a predicate applies to a set of things, and on the assumption of platonism, this set exists independently of whether it actually is the semantic value of the predicate. And if a predicate has an extension, then the extension counts a certain number.<sup>75</sup> If we accept van Inwagen’s principle, the consequence for the quantifier variantist is that she must accept that either “the number of” or “0” (or both) has a different meaning in alternative ontological languages. When the quantifier varies, so do mathematical terms.

Both options contradict our initial supposition that mathematical terms are constant in meaning. Especially the contention that there is an ambiguity in the meaning of “0” is unattractive (I come back to why below). Let’s therefore instead focus on whether it is plausible that the meaning of “the number of” is equivocal across ontological languages. First, observe that even though the number of things depends on what there is, that doesn’t imply that the meaning of “the number of” varies. Second we may wonder whether “the number of” is a mathematical term, as I’ve been implicitly assuming. This is a difficult question to answer generally, but it is often assumed that the locution “the number of” in “the number of the planets is 8” refers to 8. Furthermore, “the” and “of” are purely grammatical categories that we wouldn’t expect to change meaning between different ontological languages either, especially if we are trying to restrict the meaning-change to the quantifier. If so, only “number” is a plausible candidate for meaning variation. It is plausible to assume that “number” refers to mathematical objects, namely numbers, which are surely part of the subject matter of mathematics. So it would seem that if the quantifier variantist wants to accept van Inwagen’s principle, she will have to accept that either “0” or “the number of” (which I will continue talk about as “mathematical terms”), have different meanings in different ontological languages.<sup>76</sup>

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<sup>75</sup> This is at least true for some predicates, such as “table”. It might not hold for plural mass nouns such as “water”, but this nuance seems immaterial of the general point.

<sup>76</sup> Thanks to Einar Duenger Bøhn and Jørgen Dyrstad for correcting prior mistakes and improving this section.

One may wonder what's so problematic about that. As I pointed out above, the quantifier variantist must hold that several words change meaning when we vary the quantifier, so what makes the case of these terms any different? We can first note that the meaning-variation that was a consequence of the Eklund/Hawthorne-argument was in a domain the quantifier variantist actually makes a claim about, the physical one. Hirsch does not think that the dispute over platonism in the philosophy of mathematics is verbal (2011, p. 243f), and there is no obvious reason why quantifier variance about the physical domain would lead to meaning-variance for mathematical terms. Furthermore, the stripe of quantifier variantism that we are now concerned with is interested in restricting the meaning variance to the quantifier only. It should nonetheless be noted that this meaning-variance does not have any direct consequences for the ontology of mathematics; under the assumption of Platonism, every language will talk about the same domain of mathematical objects. My argument attempts to show that we won't be able to say that it is the *same* referential relation that holds between e.g. number-words and numbers in all languages. So it has consequences for the semantics of mathematics.

This seems like an unattractive and unwanted consequence, because even though *how* we manage to refer to mathematical objects is an unsolved puzzle in the philosophy of mathematics, we would expect it to be the same mechanism in all languages with the expressive power to do so. With the loss of a uniform treatment of how we refer to mathematical objects, the mystery of mathematics becomes even bigger. If we figure out how we manage to refer to mathematical objects in English, it will be unobvious whether that solution carries over to other ontological languages. This isn't a refutation of quantifier variantism, but imposes an explanatory demand upon the quantifier variantist that is burdensome.

The quantifier variantist's answer to this must be that mathematical terms get their meaning holistically within the language, and that mathematical terms in languages with expressive powers to talk about mathematics have different referential relations to mathematical objects. The point, according to this response, is that if a language has terms for mathematical objects, then it is obvious that one can use those number-terms to refer to mathematical objects. Whether the referential relation between mathematical terms and mathematical objects in one language is the same as in another language is not important. This response may be right as far as it goes, but again, it seems to go against the motivation behind the version of quantifier variantism that we are considering, which wants to restrict meaning-variation to the quantifier only. It is also an intuitively surprising response, for it means that also this version of quantifier variantism has consequences in other areas than the one it presumably wants to make a claim about: the ontology of physical objects.

## 2.5 Truth-conditional equivalence

We now move on to another argument against quantifier variantism. Central to Hirsch's formulation of weak quantifier variantism is the notion of "truth-conditional equivalence". As defined in chapter 1, weak quantifier variantism is the view that there are at several truth-conditionally equivalent languages in which different quantificational sentences are true. Given the intensional assumption, two languages are truth-conditionally equivalent if, for any sentence in a context in one of the languages, a speaker of the other language can, in the same context, find a sentence in her own language which is true with respect to the same possible worlds. So a sentence of one ontological language is truth-conditionally equivalent with a sentence of another ontological language if a speaker can always express the same proposition (set of possible worlds) as her ontological opponent, even though she will use sentences with different surface structure doing so. A crucial question is thus whether different ontological languages really *are* truth-conditionally equivalent – can they describe the same possibilities? Based on this overarching question, we can formulate two sub-questions:

- (i) For a quantificational sentence,  $S$ , of one ontological language, is it always possible to find a truth-conditionally equivalent sentence (i.e. a sentence expressing the same proposition),  $S_1$ , in another ontological language?

That is, are the different ontological languages we have been considering really truth-conditionally equivalent? To determine (i), we first need to answer this question:

- (ii) How do we evaluate whether two sentences in alternative ontological languages are truth-conditionally equivalent?

Let us therefore try to answer (ii) first. To evaluate whether two sentences are truth-conditionally equivalent, we must determine whether they are "true with respect to the same possible worlds" (Hirsch 2011, p. xi). By standard referential semantics this is quite simple: we evaluate whether the sentences talk about the same objects having the same properties, standing in the same relations, and so on. Quantifier variantism makes this evaluation harder, for we don't necessarily have the same objects to compare the properties of. The quantifier variantist is committed to the notion that truth-conditionally equivalent sentences that appear to imply the existence of different things describe the same possibilities. The possible worlds themselves are of course unaffected by the different descriptions of them, but when we have systematically different descriptions it is harder to know whether two sentences are true with

respect to exactly the same possible worlds. This is the basic problem with quantifier variantism when it comes to answering (ii).

Ontological views are assumed to be necessary truths, so while a nihilist will describe the possible worlds as containing only simples, the universalist describes the possible worlds as including all sorts of composite objects. From the perspective of a given language, the mereological and persistence properties of the possible worlds vary. This much follows from quantifier variantism. Despite this, and as (i) makes clear, it is crucial that the two views describe the same possible worlds equally well. This claim seems made from a view from nowhere, by someone who simply knows that the languages describes the same possible worlds, but by trying to answer (ii), we seek an explanation of just how we know this.

Two sentences are truth-conditionally equivalent because they express the same proposition. But then a proposition is a set of worlds without us having any further grasp of what the possible worlds are like. Given that the different ontological languages can describe these worlds equally well using different quantificational apparatus, the worlds themselves seem underspecified, insufficient to illuminate whether the sentences really are truth-conditionally equivalent. Before we have some *quantificational grip* on the possible worlds (and a unified stock of names and predicates) they are explanatorily idle with regard to answering our question. We seem to want a quantificational scheme imposed (i.e. the possible worlds described by a language) to evaluate whether the languages are truth-conditionally equivalent, or at least, we need something more than a reference to the sentences being “true with respect to the same possible worlds” to answer (ii).

To exemplify the difficulties in answering (ii), consider these allegedly truth-conditionally equivalent sentences:

There are tables.<sub>Biglish</sub> (3)

There are particles arranged tablewise.<sub>Smallish</sub> (4)

and:

In 1860, Abraham Lincoln had a temporal part that was bearded.<sub>Perdurance</sub> (5)

In 1860, Abraham Lincoln was bearded.<sub>Endurance</sub> (6)

What is the relationship between (3) and (4), and (5) and (6), in virtue of which they are truth-conditionally equivalent? What I hope to have shown in this section so far, is that talking about the possible worlds themselves won’t get us any further in answering this questions. For on the absence of a common set of objects to appeal to when comparing the truth-conditions



of the sentences, it is unclear how we know that they express the same propositions. That (3) and (4), and (5) and (6), hold true with respect to the same possible worlds is merely a description of the problem. By which criterion are we supposed to evaluate it?

As we have seen, truth-conditional equivalence comes to being “true with respect to the same possible worlds” (Hirsch 2011, p. xi). Perhaps we can get some help answering (ii) by analyzing this relationship further. One way, the least demanding way, of analyzing this is simply as being necessarily equivalent, that is holding in all the same worlds. But the sentences “Nothing is both red and blue all over” and “Everything is self-identical” are true with respect to the same possible worlds, but they won’t do as “truth-conditionally equivalent sentences” in the required sense (Eklund 2007). Furthermore, these sentences hold true with respect to the same possible worlds because they are true in *all possible worlds*. This explanation is easy to grasp, but in the case of sentences about physical objects, which are contingent, how do we *know* that the sentences are true with respect to the same possible worlds? This criterion doesn’t seem to give us much help.

Necessary equivalence is the weakest relation that can hold between truth-conditionally equivalent sentences. The strongest is synonymy. But this won’t do either, for one can hardly say that (3) and (4), and (5) and (6), are synonymous. The sentences belong to different languages, which would seem to disqualify them as synonyms. A final suggestion, intermediate in strength, is that the sentences are *a priori* necessarily equivalent. In a book preceding his writings on quantifier variance (but not unrelated to the it), Hirsch defines *a priori necessary equivalence* as: “Two sentences are [a priori necessarily] equivalent if someone who understands both of them can know a priori that they hold true in exactly the same possible situations” (1997, p. 4). In one sense, this is better. If we simply by understanding the sentences *know* that they hold true with respect to the same possible worlds, then our question is answered. The answer to (ii) is that *understanding* the sentences is enough. On the other hand, this still has the problem of counting sentences that are intuitively not truth-conditionally equivalent as equivalent, such as “Nothing is both red and blue all over” and “Everything is self-identical,” for we know a priori that they are true in the same possible worlds. But leave such problems aside, let’s look closer at a priori necessity.

One problem with this characterization of the relationship is that it doesn’t give us all we want; it doesn’t tell us in virtue of what the languages describe the same possible worlds. It simply postulates understanding the sentences. An example of a satisfactory explanation was given above: if the sentences refer to the same things, uses the same predicates and so on, then we know why the sentences are truth-conditionally equivalent. But this explanation is not available to Hirsch, for the objects referred to and meaning of predicates plausibly changes

between the ontological languages. The general problem is that we don't have a uniform notion of "truth-condition" to use to compare the different languages and persuade ourselves that the sentences hold true with respect to the same possible worlds. Referential semantics provides a uniform treatment of the truth-conditions of any language (cf. (T) above), but Hirsch must appeal to our holistic understanding of the languages and in virtue of this, we come to know that the sentences hold true in the same possible worlds. For those generally skeptical of holistic meaning theories, this might be a reason to reject Hirsch's explanation.

Even if one is of the opinion that there are no problems with the holistic character of Hirsch's answer to (ii), there are other problems with the suggestion. It is not at all obvious that we know *a priori* that the sentence "Abraham Lincoln had in 1860 a temporal part that was bearded" in the language of the perdurantist and "Abraham Lincoln was bearded in 1860" in the language of the endurantist necessarily hold true of the same possible situations. Given the big difference between the alternative languages, it is unclear that we understand the two languages well enough to evaluate whether they hold true in exactly the same situations. I cannot speak for others, but I find it unobvious that the sentences are truth-conditionally equivalent. We may bring in some witnesses here: David Lewis and Peter van Inwagen accepted different theories of parthood, and by Hirsch's lights, spoke different languages. But they nonetheless engaged in the dispute and rejected the notion that the languages they used were truth-conditionally equivalent. It can also be noted that for philosophers who reject the *a priori*, Hirsch's notion of truth-conditional equivalence evaporates.

I've tried to argue that there are several aspects of Hirsch's suggested answer to (ii) that make it hard to know exactly how to evaluate whether two ontological languages are truth-conditionally equivalent. To me, it seems like somewhat of a mystery how we know this. But let's assume that we have a good enough grasp of how we evaluate whether two sentences are truth-conditionally equivalent. To determine whether two sentences are truth-conditionally equivalent, we also need to know what the possibilities are. We need a conception of what the possibilities are to claim that alternative ontological languages describe the same possibilities. Question (i) asks whether it is always possible, for any sentence of an ontological language, to find a truth-conditionally equivalent sentence in another. But as truth-conditional equivalence is a question of expressing the same propositions, which are individuated by possible worlds, the two languages must operate with the same set of possibilities. This is a potential problem, because some ontological views reject some distinctions of possibility that the other does not.

I now wish to argue that Hirsch overlooks the fact that different ontological theories take there to be different possibilities. To formulate a notion of truth-conditional equivalence,

Hirsch must presuppose that the different ontological languages operate with the same set of possible worlds. But this assumption is proven unwarranted if we look closer at how ontology actually proceeds, thus undermining the project of deflationism.

We may get a hang on this by asking: Why engage in ontology? One reason is that one is curious about what there is. Another reason is that one is curious about what's possible. For, as Hawthorne puts it, different ontologies "multiply possibilities in ways that are resisted by other ontologies" (2009, p. 221). Ontological views may have consequences for what the possibilities are. As we have seen, quantifier variantism requires that different ontological languages describe the *same* possibilities. They reflect different ways of describing the same possibilities, but because the languages are truth-conditionally equivalent, they must, by Hirsch's definition of truth-conditional equivalence, describe the same worlds. But ontological theories sometimes have implications for which possibilities there are. Thus, the argument goes, the quantifier variantist has made an unwarranted assumption on behalf of the different ontological position she claim are truth-conditionally equivalent; that they operate with the same possibilities.

To argue for this, I will give two examples in the ontology of physical objects where the different ontological views have different consequences for what the possibilities are. The argument is based on Hawthorne (2009). The argument is successful if we can point to distinct possibilities countenanced by one ontology, and rejected by another.

Let us start by considering the famous rotating disc argument of Kripke<sup>77</sup> and Armstrong (1980). The argument is thought to have bearing on the dispute between endurantists and perdurantists, because the perdurantist has trouble explaining the difference between two intuitively distinct possibilities that the endurantists accept. Imagine a perfectly homogenous disc, i.e. a perfectly spherical disc of homogenous matter, so that no parts of the disc can be distinguished by their intrinsic properties. It would seem that we can distinguish between a scenario in which the disc is stationary and a duplicate disc rotating. The endurantists can quite easily distinguish these possibilities. She can identify the rotating disc as the one where the same segment of the disc is at different locations at different times, and the stationary disc as the disc where the same segment is at the same location at different times. Thus the endurantists uses the identity-over-time of the same segment to distinguish the possibilities. In contrast, it is widely assumed that there is a problem for standard perdurantism to distinguish these possibilities (e.g. Sider 2001b, p. 225). For it would seem that the relationship between the different temporal parts are the same whether the disc is rotating or not. As the perdurantist cannot appeal to the identity-over-time of the same segment, but only relations between

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<sup>77</sup> Kripke has only presented the argument in lectures and unpublished manuscripts, so I base what's said here on Callender (2001).

the different instantaneous temporal parts of the disc, she seems without resources to accommodate this distinction of possibility. Thus on the face of it, the perdurantist must accept that a possible world with a stationary disc is the same possible world as one with a spinning disc.

In response to this argument, different modifications and additions have been made to perdurantism. Some have brought in causal notions to explain the differences,<sup>78</sup> this has been criticized,<sup>79</sup> and so the discussion goes. There is no point in considering all the possible responses here, for the general point is that to determine whether the two ontological positions describe the “same possibilities”, one must *engage in ontology*, and it is just such engagement Hirsch’s flavor of deflationism attempts to avoid. Furthermore, one reaction to this argument is to bite the bullet and *not* countenance the possibilities the endurantist distinguishes. So for instance Sider (2001b, p. 225f) tries to distinguish the world with the rotating disc and the one with the duplicate stationary disc by appealing to differences in the environment of the discs. But, he says, if the worlds cannot thus be distinguished, he will accept that the possibilities aren’t distinct (Sider 2001b, p. 233f). So if the perdurantist cannot distinguish a world with a rotating disc and a duplicate world with a stationary disc, Hirsch’s Perdurant-English and Endurant-English aren’t truth-conditionally equivalent after all.

I will consider possible replies after I’ve presented the next case where ontological views accept different possibilities. But we may note straight away that if the perdurantist and endurantist disagree on the possibilities, then their languages cannot be truth-conditionally equivalent. This might either be cashed out as the ontological disputants operating with different sets of possible worlds, or we may interpret them with a common set of possible worlds in which case their languages aren’t truth-conditionally equivalent after all. In any case, this apparently shows that weak quantifier variantism is false for this dispute.

Another example where ontological positions will have different views on what the possibilities are is in the dispute over composition. For instance, Sider (1993) argues against van Inwagen’s (1990) view that only simples and living organisms exist by showing that van Inwagen’s position rejects the possibility of gunky worlds, i.e. worlds where the composite objects aren’t composed of mereological simples. Possible responses to this argument notwithstanding, it shows that ontological theories sometimes have consequences for which possibilities there are, and thus that we cannot simply assume, as Hirsch does, that there is a fixed set of possible worlds to operate with when defining truth-conditional equivalence. Again, the

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<sup>78</sup> See, for instance, Russell (1914, pp. 114–116) who claims that a sequence of temporal parts counts as a persisting object only if the sequence falls under a causal law; Armstrong (1980, p. 77) claims the earlier parts of the sequence must be “nomically required” for the later parts for it to count as an object.

<sup>79</sup> Zimmerman (1998, p. 270–274) argues that the sort of causal relations required to distinguish the possibilities are incompatible with the thesis of Humean Supervenience (Lewis 1986, p. xi), and in general that one must accept a *sui generis* notion of causation to accept the causal/nomic reply to the rotating disc argument.

problem is that one ontological position countenance more possibilities than another. Hirsch cannot accommodate this, because the different ontological positions must merely reflect different descriptions of the same possibilities.<sup>80</sup>

Hirsch can attempt to reply that ontologists in general won't accept these differences of possible worlds, but rather try to converge on which possibilities there are. So for instance, as we have seen, some perdurantists try to find ways to distinguish worlds with a rotating homogenous disc and a stationary homogenous disc. But this reply simply ignores how ontology proceeds and what ontologists are up to. By adopting *sui generis* causal notions the perdurantist can account for the same possibilities as the endurantist, but this may be in conflict with her commitment to Humean Supervenience (see, for instance, Sider 2001b, p. 225f). Such interconnections have relevance for which possibilities the different ontologists claim there are. It would seem that Hirsch must take a stance on a number of issues in ontology to at all be able to have a fixed set of possible worlds for the sentences to be true with respect to, and this is part of what's at stake in ontology.

May the quantifier variantist object that her arguments are supposed to prove *just that* the different ontological views operate with the same set of possible worlds? To talk about the possibility of "truth-conditional equivalent alternative ontological languages" does no such thing. Usually in philosophy, it is considered a substantive question just which possibilities there are (consider the debate over whether there are zombie-worlds after Chalmers 1996). Even though philosophers would generally attempt to countenance all the intuitive possibilities, it would seem implausible that Hirsch's arguments on general grounds show that ontologists necessarily operate with the same set of possibilities.

In general, Hirsch must take a stance on what the possibilities are without regard for what the ontologists actually disagree about. But as we have seen, ontologists sometimes disagree about possibility, and given that it is not a trivial question just which possibilities there are, Hirsch cannot simply assume that ontologists describe the same possibilities.

We have seen that Hirsch has trouble with giving a plausible account of how we know that two sentences are truth-conditionally equivalent. One part of the problem is that it is unclear how we evaluate whether two sentences are truth-conditionally equivalent in the first place, another problem is that ontologists disagree on what the possibilities are. Hirsch's account of truth-conditional equivalence essentially turns on there being a unified set of possibilities that all ontologists talk about, but without engaging in the ontological disputes them-

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<sup>80</sup> Hawthorne mentions another example of a possibility that the universalist might accept, but that is likely to be rejected by the nihilist: "If one accepts the possibility of pairs of worlds that differ *de re* at the level of certain macro objects but are alike – qualitatively and *de re* – at the level of simples, then one will think the nihilist's language intensionally too coarse grained" (2009, p. 222).

selves, and taking a stance on which theory actually is true, Hirsch cannot make this assumption. Thus we cannot avoid mainstream ontology by retreating to quantifier variantism.

## 2.6 Differences

In the following, I assume that quantifier variantist must reject (T), because I know of no other good replies to the semantic arguments of section 2.3. The rejection of (T) and embrace of the variation of semantic categories are examples of consequences of quantifier variance that requires us to revise our views on language. In sum, quantifier variantism entails quite radical divergences from traditional views on language. This section concludes this chapter. While it doesn't contain any direct arguments against quantifier variantism, I point out some further consequences of quantifier variantism that show how the view differs from traditional views on language and quantification. It is a contention of this section that we believe we understand quantifier variance better than we in fact do. The differences between ontological languages that I point out here will be relevant for the argument of the next chapter.

In section 1.3 I described two conventions to go from speaking like a perdurantist to speaking like an endurantist, and vice versa. Those conventions were from Hirsch (2009), and exhibited an asymmetry between going from a language with what we may call, for now, a more “permissive ontology”, such as universalism or perdurantism, being true, to a language in which a more “restrictive ontology”, such as nihilism or endurantism, is true. When going from a “big language” to a “small language” (a change from “big to small”), the suggestion was that we simply “restrict our quantifiers”. But when going from a small to a big language, Hirsch provided a more elaborate convention. Some philosophers have focused on this asymmetry, finding it more pressing to give an explanation of how we may go from small to big, than the other way around (see for instance Dorr 2005). Philosophers have found the change from big to small rather unproblematic because it appears quite similar to a familiar semantic phenomenon from natural language, quantifier domain restriction, but we have no corresponding phenomenon in natural language when going from small to big.<sup>81</sup> However, I argue that the shift from big to small is as radical as a shift from small to big. The take home message is that the semantic mechanisms involved in quantifier variance are more alien than is usually assumed.

First, note that natural language quantifier restriction is a function of the context. When someone asserts “Every bottle is empty” on a given occasion, it is unlikely that it means that every bottle in the universe is empty. Rather, the speaker presumably intends to restrict

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<sup>81</sup> That one can lift the already imposed quantifier restrictions won't do. For both parties to the dispute claim to be using an unrestricted quantifier.

the domain of quantification on this occasion to some subset of the universal domain, and this is accommodated in interpretation by contextually restricting the domain (Stanley and Szabo 2000). Exactly how the context contributes to interpretation to arrive at the correct quantifier domain restriction is disputed, but there is little doubt that the context plays an essential role. When it comes to the change from big to small, one is “restricted” to the new, smaller domain in *any* context. When Pedro restricts his quantifiers, this is supposed to be a permanent feature of his language. What we might call “ontological domain restriction” is not a function of the context, but must be inherent in the semantics of the whole language. Thus the familiar explanations of how natural language domain restriction gets operative in a given context are inadequate to explain the phenomenon of ontological domain restriction.

Furthermore, natural language domain restriction presupposes a bigger domain. If there is no bigger domain, there is no quantifier restriction. If one quantifies unrestrictedly in natural language, then by definition there is no quantifier domain restriction.<sup>82</sup> This is not the case with ontological domain restriction, where the idea is that the new “restricted” domain is supposed to be the new universal domain – ontologists intends to quantify unrestrictedly. Relatedly, with natural language restriction one can loosen the bonds of the restriction, or lift it completely, by saying “No, I don’t only mean that every bottle in fridge is empty, I mean that every bottle in the universe is empty”. In contrast, the consequence of a successful language-shift (which is a more apt description of the phenomenon) is that the new unrestricted domain of quantification simply doesn’t contain the objects one earlier quantified over.

Finally, and perhaps most interestingly, while an ontological domain restriction would seem to change the meaning of predicates, a natural language domain restriction does not. The first sense in which this happens was illuminated in the reply to the Eklund/Hawthorne argument; quantifier variantism seems to imply that the semantic categories of name, predicate and reference change between ontological languages. But there is also another sense in which predicates change meaning (which was briefly mentioned in section 2.3). If we assume an orthodox treatment of the semantics of predicates, the meaning of a predicate is given by the extension of the predicate, i.e. the set of things satisfying the predicate. The meaning of “red” is the set of red things, the meaning of “mammals” the set of mammals. Now consider natural language quantifier domain restriction: If the meaning of “red” varied with the domain restriction, it would entail a problematic semantic instability. If the domain is restricted to the things in my apartment, the sentence “I have touched every red thing” is true, because the

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<sup>82</sup> This statement ignores the controversy over whether it is possible to quantify (absolutely) unrestrictedly (see Rayo and Uzquiano 2006). It seems to me that we can safely ignore those problems because we are here talking about rather mundane quantification over physical objects.

domain is restricted to a subset of the red things. A case of innocent quantifier domain restriction doesn't alter the meaning of "red" or any other predicate. This is different in an ontological quantifier restriction: If we go from English to Smallish, "red" would get a new meaning, because the set of red things has changed (there is no such things as my red copy of *Metametaphysics*). This kind of meaning-change is foreign to the natural language case. Based on these three examples of differences between the phenomena, I conclude that there is no clear relationship between them, except that we in a sense go from a bigger to a smaller domain to quantify over.

However, can we actually characterize the shift from a big language to a small language as just that; a change from "big to small"? In one sense we can, and in another sense we cannot. The sense in which we can: For every thing, we can count it, and because the universalist believes in all the simples the nihilist believes in, and in addition all mereological sums of these, when Big counts all physical things he will arrive at a number much higher than Small. In this language-internal sense, we can say that Big's language is much bigger or more permissive. Thus the "number of things" is bigger. But this masks the fact that "thing" has changed meaning. For the two languages describe the same world equally well, and because they operate with different concepts of "thing", we cannot directly compare the numbers they arrive at when counting everything. Thus it would seem that the quantifier variantist would have to give up the notion that the world has a fixed cardinality. In this connection, it is interesting to quote Tim Maudlin: "If there are no objective facts about the comparative character of objects, we must fall back into the unpalatable position that the only real structure of the universe is its cardinality" (2007, p. 84). There isn't even any real cardinality-structure on the quantifier variantist's picture of reality, which is intuitively surprising. In any case, it will be convenient to talk about the "size" of languages also in the following, but keep in mind this caveat.

This section shows that what I will call, for lack of a better word, the *difference* between alternative ontological languages is bigger than we first have expected. As noted, in rejecting (T) a speaker of Smallish must reject that Big's sentence "*a* is a table" is a subject-predicate sentence, that "*a*" is a name that refers and also that "table" in Biglish has the same meaning as "table" in Smallish. These terms are language-specific, and there is no obvious way to close the gulf between the languages. Further differences between different ontological languages are that predicates change meaning when we shift language, and that we cannot directly compare the number of things because this is a language-internal issue. Quantifier variantism has wide-ranging consequences for how we conceive of world and language (A topic not treated here is that it also implies vagueness of identity, see Hirsch 2011, p. 45–67). The difference between different ontological languages is bigger than we would expect.



## 3 THE ARGUMENT FROM CHARITY

### 3.1 Overview

In chapter 1, the argument from charity was formulated as two conditionals:

- (iii) If ontological disputants are speaking plain English, then ontological claims diverging from common sense judgments about what there is are trivially false.
- (iv) If ontological disputants are not speaking plain English, then they are speaking distinct ontological languages, asserting trivial truths in their own ontological languages. In this case, the dispute is verbal.

The antecedents of the conditionals contradict each other, so one of them is true. Both consequents deflate ontology, so if one of the conditionals is non-vacuously true, ontology is deflated. To avoid the deflationary conclusion of the argument from charity one must therefore accept one of the antecedents, and deny the corresponding consequent.

As was pointed out in chapter 1, the truth of (iv) presupposes weak quantifier variantism. In chapter 2, I provided arguments against weak quantifier variantism. If those arguments are any good, we can deny that ontologists are speaking different ontological language, effectively undermining (iv). However, rejecting (iv) on this ground doesn't say anything about which language ontologists in fact *are* speaking. As English (or some other natural language)<sup>83</sup> is the ordinary language of ontologists, the most conspicuous candidate for the language of ontologists is English supplied with technical vocabulary. But if ontologists use English, then the truth of (iii) entails that non-commonsensical ontological claims are simply false. Thus rejecting weak quantifier variantism does not suffice to avoid the argument from charity. Accordingly, quantifier invariantists who are concerned about the argument from charity will have to take on (iii).<sup>84</sup>

Even if one does not find the arguments of chapter 2 persuasive, and accept (or are agnostic about) weak quantifier variantism, the best way to challenge the argument from charity is to take on (iii). For the best answer to the argument from charity is to hold that ontologists speak plain English, and simultaneously reject the consequent of (iii). This might be thought to go against what I've said about the argument from charity presupposing weak quantifier

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<sup>83</sup> In the following, I will omit the qualification "or some other natural language". This is simply to make the presentation easier to read.

<sup>84</sup> This is barring the change to an ontologically privileged language, the existence of which is controversial. These issues are discussed in chapter 4, and I will generally not include the caveat "barring the change to an ontological privileged language" when discussing the options for rejecting Hirsch's conclusions in this chapter.

variantism. This thought is unwarranted. If one rejects weak quantifier variantism, one has avoided the argument from arbitrary languages, which is a deflationary threat in itself. Furthermore, if weak quantifier variantism is true, it is easier to argue that ontologists speak different languages, which entail that they are having a verbal dispute. So those who accept weak quantifier variantism but wish to avoid the conclusions of the argument from charity will then have to provide independent arguments for why it is plausible that ontologists actually speak the same language. I provide such independent arguments in section 3.2, but this exercise is unnecessary for those who already reject weak quantifier variantism, for there is only English to speak. It can also be noted that a reason for focusing on the truth of weak quantifier variantism is that (iv) is the horn of the dilemma that has received most attention. In the literature on Hirsch's arguments, it appears that philosophers haven't worried too much about (iii).<sup>85</sup> This is in itself a good reason to consider (iii) in detail. In any case, to provide a full treatment of Hirsch's arguments, which is what I'm attempting, both conditionals must be addressed.

A final and independent reason for focusing on the standing of ontology in English is that English has its own ontological tensions that we would like to disentangle. Many of the problems ontologists are concerned with – such as the question of how two objects may occupy the same region of space, the problems of composition, and the paradoxes of persistence – all arise in English.<sup>86</sup> Thus we would like, on independent grounds, a solution to these puzzles. Hirsch recognizes that there are tensions in English that generate the problems ontologists are concerned with and that they must be addressed, but doesn't think such tensions has any bearing on the truth of common sense ontology (2011, p. 232). If the arguments of this chapter are successful, they vindicate ontology conducted in English.

In conclusion, accepting the pre-theoretical claim that ontologists are speaking English is the best option for those who want to avoid the deflationary consequences of the argument from charity. In deference to the possibility that weak quantifier variantism is true, this chapter will first argue that ontologists speak plain English (section 3.2). This might seem like a silly thing to argue for, but is, as I hope to have made clear, important to answer the argument from charity by affirming the antecedent of (iii). By arguing that ontologists speak English, I indirectly argue that they don't speak different ontological languages. I then argue that it is not the case that non-commonsensical ontological claims are "trivially false" in English, effectively

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<sup>85</sup> Some of the papers dealing with (iv) are Bennett (2009), Dorr (2005), Hawthorne (2009), Jackson (2013), McGrath (2008) and Sider (2009). As far as I know, no published paper discusses (iii) directly, but Hawthorne (2009, p. 218f) makes some remarks about the issue.

<sup>86</sup> Or so it seems. As far as I know, no one has argued that one starts speaking another language as soon as one recognizes these tensions.

denying the consequent of (iii) (section 3.3). Together, this amounts to arguing against (iii). Section 3.4 deals with some loose ends. If my arguments are sound, they leave open the question of which quantificational sentences are true in English. This is a question for ontology.

### **3.2 Ontologists are speaking plain English**

So what are the reasons to believe that ontologists actually conduct their inquiry in English? First, philosophers who engage in ontology in international philosophy journals are either native English speakers or have at some point learned English. They write their non-ontology papers in English, and the words and sentences of their ontology-papers appear to be English words and sentences (supplemented with technical vocabulary). Despite this, Hirsch (2005, 2009) claims that philosophers doing ontology are really speaking ontological languages that aren't English. This has a presumption against it. Excluding the rare occasion of a philosopher explicitly announcing that she isn't speaking English, it would on the face be uncharitable to assume that philosophers have tacitly, and without recognizing it, started speaking an artificial ontological language. The most natural assumption is that ontologists speak English. Below follows three more arguments for this conclusion.<sup>87</sup>

#### **3.2.1 Difference between ontological languages**

If ontologists aren't speaking English, they are speaking different ontological languages. But as I argued in section 2.3 and 2.6, the *difference* between English and alternative ontological languages is vast, so it is implausible that ontologists unintentionally and inadvertently suddenly start speaking different languages. Based on the Eklund/Hawthorne-argument I argued that the meaning of both quantifier and predicates systematically varies between different ontological languages. A plausible principle of language-change is that the smaller the difference between the languages, the easier it is to slip into speaking a different language. When we consider these big differences between ontological languages, it is implausible that ontologists should inadvertently "slip into" speaking different ontological languages. Hirsch believes that van Inwagen and Lewis spoke different ontological languages when they disagreed about which composite objects there are. But in that case, he would have to say that they unintentionally ended up using different names and predicates in the dispute. That's intuitively not likely. There is a significant difference between starting to use a certain *term*, such as "cup", with a non-conventional meaning (e.g. such that it also applies to glasses), and starting to speak a different ontological language.

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<sup>87</sup> Hirsch (2005, 2009) thinks that there are good reasons to believe that ontologists speak different ontological languages because of considerations of charity, see section 1.4. However, I argue indirectly in section 3.3 that those reasons doesn't support such a conclusion, because Hirsch's application of the principles of charity is unsatisfactory.

In section 2.6 we saw that the semantic mechanism involved in changing between ontological languages is semantically unfamiliar, as it isn't a close cousin of natural language domain restriction. This would seem to increase the height of the hurdle for a language change. Simply put, an act of considerable semantic power would seem necessary to change ontological languages, but real life ontologists haven't even attempted the feat.

It can be objected that different ontological languages are systematically related to English, and thus that the hurdle is easier to overcome than I contend. In reply to this, I want to direct the objectors attention to the fact that if ontologists unwittingly slip into using alternative ontological languages, they end up using a wide variety of predicates with a new meaning, and also with a different meaning from one's linguistic community at large (see 3.2.3 below). In the absence of an explicit linguistic act to this effect, I find it hard to believe that English speakers inadvertently change languages in the way Hirsch's view requires.

### **3.2.2 Charity to retraction is overlooked**

The two considerations I've so far given in favor of believing that ontologists speak plain English have been independent of Hirsch's arguments. I now want to consider Hirsch's positive reasons for rejecting the presumption that ontologists speak English.

In at least three papers, Hirsch has argued directly that ontologists are speaking different ontological languages (the antecedent of (iv)), which implies that at least some of them are not speaking English (2005, 2008a, 2009). If this argument is incorrect and doesn't warrant the intended conclusion, one (indirect) argument against the claim that ontologists are speaking English is dispensed with, and I don't know of any other arguments against this claim.

The argument for the antecedent of (iv) that I presented in chapter 1 was that a correct linguistic interpretation of ontologists implies that they are speaking different languages. Here I argue that Hirsch's "correct linguistic interpretation" suffers from an incomplete application of the principle of charity, and thus that we don't have good reasons for believing the conclusion which is solely based on the principle of charity.

Recall that charity to retraction is the constraint on interpretation that says interpreters must consider whether speakers will retract their utterances in the face of additional evidence. If there is a high probability that ontological disputants will retract their positions in light of additional evidence, then the contention that ontologists are speaking different ontological languages is undermined. For we conclude that they speak different languages when the alternative is to impute widespread confusion and false belief. But that is not the only alternative if the dispute is of a kind that requires retraction under rational constraints and we can expect the disputants to react rationally and quite uniformly to new evidence. In that case,

the disputants agree on more or less clear conditions under which either party would recognize her claims as false or not justified, and consequently act upon it. It is not uncharitable, then, to impute the same meaning to both parties in a situation of agreed upon rational constraints, since given the evidence at the time of interpretation, they both behave rationally.<sup>88</sup>

On the face of it, ontologists, as the more or less rational inquirers they aim to be, would be expected to retract their positions if faced with new evidence. This undermines the argument that they are speaking different languages if we consider charity to retraction, which Hirsch notes may immediately show that a dispute is substantive (Hirsch 2005, p. 74). If ontologists are expected to react in the same way to new evidence, if not retracting their positions then at least address the argument and provide responses, they share a stock of evidence, and there are some rational constraints governing their practice. It is plausible to suppose that when we interpret ontologists, we must pay attention to charity to retraction.

What does Hirsch have to say about the relevance of charity retraction when interpreting ontologists? Hirsch does not argue that the disputants will not retract their positions. Rather, he assumes that they will not. The assumption is that ontology has reached a stage where ‘all is said and done’, but where there still is disagreement:

What about charity to retraction? Should that also play a role here? Of course ontologists do occasionally retract their positions, but, as Lewis remarks, a stage seems eventually to be reached in ontology when “all is said and done,” when “all the tricky arguments and distinctions and counterexamples have been discovered,” so that each position has achieved a state of “equilibrium.” I’m assuming that in the ontological disputes under discussion the ‘all is said and done’ stage has been reached. (Hirsch 2011, p. 159)<sup>89</sup>

In a later paper, Hirsch says that his method for deflating ontological disputes applies only if the dispute is at the “all is said and done”-stage (2011, p. 231). That ontology is in a state of all having been said and done is a premise in Hirsch’s argument from charity. But do we have good reasons to believe that it is?

An “all is said and done”-stage is a stage where the ontologists do not change their views: “The ‘all is said and done’-point has been reached when ontologists have gone around the dialectical block enough times to feel secure that they are prepared to reject any [metaphysical principle] that might undermine their ontological assertions” (Hirsch 2011, p. 161). The “all is said and done”-assumption is what justifies not considering charity to retraction.<sup>90</sup> Thus

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<sup>88</sup> Thanks to Reier Helle here.

<sup>89</sup> Hirsch is quoting Lewis (1983a, p. x).

<sup>90</sup> If we can ignore charity to retraction when interpreting ontological disputants, Hirsch contends that the disputants should interpret each other as speaking truthfully in their own language (2005, 2009). I won’t take a stand on this. McGrath (2008) disputes it. He thinks Hirsch has overlooked an important element of charity.

the crucial question is whether there are good reasons to believe that ontological disputes have reached such a stage.

Publications on composition, temporal parts, coinciding objects, and related meta-questions do not seem to be stopping any time soon, so it seems as if the ontologists themselves think that there are still arguments to be made over these topics, that is to say, they do not believe that all is said and done. In general, one would expect ontologists to notice if all is said and done, for then there is nothing more to argue about. Though there may be exceptions to this, this seems like a *ceteris paribus* plausible claim. The same seems to hold for verbal disputes: one would in general expect people to eventually discover that they are engaged in a verbal dispute.<sup>91</sup> Thus Hirsch's assumption of "all is said and done" in ontology is not supported by the most obvious source of evidence for it.

Here is a suggestion of how Hirsch may try to argue for his assumption: Ontology as currently practiced is, as a matter of empirical fact, in a state where "all is said and done" between the disputants because ontologists don't retract their positions.

This is a distinctively *empirical* claim, perhaps a little unfamiliar in mainstream philosophy. However, metaontology sometimes have to venture into the empirical. If there is any support of the assumption that "all is said and done" in Hirsch's writings, it is the claim that ontologists rely on 'ontological axioms' in their theorizing (2011, p. 164–164). Here are two of Hirsch's examples of such axioms (2011, p. 165):

- a) A composite thing must have causal powers beyond the causal powers of its parts.
- b) It cannot be indeterminate whether two things compose a third thing.

These propositions are certainly appealed to as premises in arguments in ontology. But are appeals to such basic principles a symptom of a dispute where "all is said and done"? Hirsch suggests so by describing how he takes ontological disputes to work: '[E]ach camp will try to defend its position by appealing to its favored axioms. The axioms themselves can't be defended – they are, so to speak, the bottom line for each camp' (Hirsch 2005, p. 80).

In general, Hirsch does not come close to establishing this as a plausible descriptive claim about the discipline of ontology. Given many metaphysicians' commitment to mainstream metaphysics, where holistic theoretical considerations determine our acceptance of propositions and principles with ontological import, I believe it is false to claim that ontologists treat principles like (a) and (b) as (close to) axioms. While it would be simplistic to say that these principles by themselves are used to reach conclusions in ontology, it is fair to say

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<sup>91</sup> Chalmers (2011) suggests one method for identifying verbal disputes. When applied to disputes in ontology, Chalmers (2009) finds that such disputes are not verbal, so the parties are not using distinct languages.

that (a) plays a central role in, for instance, Trenton Merricks' (2001, chapter 3) 'overdetermination argument' against the existence of inanimate objects such as tables and baseballs, but Merricks surely seems to *argue* for its truth (2001, p. 56–84). Ted Sider applies something like (b) in an argument for four-dimensionalism, but the discussion of (b) is comprehensive and far from giving the impression that (b) is treated as an axiom (2001b, p. 120–133).

Furthermore, here is an empirical observation that supports the claim that ontologists in fact retract their views: One of contemporary ontology's main contenders, Ted Sider, embraces nihilism in one of his latest papers (2014), although he argues directly against it in Sider (1993), whilst Sider (2001b) defends universalism. Sider has retracted his position. So we can't, without further argument, assume that all is said and done and that ontologists won't retract.

Thus I think Hirsch is wrong in his description of how ontology unfolds. In any case, he should provide much more evidence for the claim that all ontologists are 'prepared to reject any axioms that might undermine their ontological assertions' (Hirsch 2011, p. 160). Just as I pointed to Sider, Hirsch should provide some empirical evidence for his empirical assumption that ontological disputes are at a stage where all is said and done.

Is it shown by considerations of charity to retraction that ontologists are speaking the same language? Hirsch can object that, indeed, there is *some* retraction going on, but that it is too little, and that it is outweighed by other interpretative considerations. In response to this, I insist that the burden of proof is on Hirsch with regard to whether there is too little retraction. As far as I know, ontologists quite conscientiously evaluate arguments against their positions and occasionally retract. The fact that ontology is difficult should not be considered evidence that ontologists are speaking different languages. Second, even though I think charity to retraction is sufficient (at least together with the other arguments of this section) to show that ontologists speak English, those who disagree are invited to evaluate my arguments about other elements of charity in section 3.3. There I argue that charity to perception and understanding create little pressure to interpret English blindly in accordance with what people are inclined to say. If those arguments are sound, then there are few considerations of charity left to outweigh charity to retraction. Overall, considerations of charity don't imply that ontologists speak different languages.

Could Hirsch object that he isn't in fact making an empirical claim about the present status of ontology, but rather a claim about a hypothetical 'all is said and done'-stage, where there is no retraction, but where the ontologists nonetheless disagree (as opposed to agree, in which case they presumably speak the same language)? Hirsch clearly thinks that there is no substance to the debate, so everything is always said and done. If this is the ground for the

assumption, Hirsch is simply presupposing one of his controversial deflationary conclusions. A strong claim like this requires strong arguments, and it seems Hirsch hasn't provided any.

In conclusion, ontologists' common retraction, or at least their shared propensity to evaluate arguments with potential bearing on their ontological positions, suggests that all isn't said and done. As we saw, this was a premise in Hirsch's argument for why charity to retraction is irrelevant when interpreting ontologists. I suggest that we must consider charity to retraction, and that, together with the arguments of section 3.3, this suggests that ontologists speak English. They are engaged in a contrived dispute, yes, but supposedly not a verbal one.

### 3.2.3 Anti-individualism

A final argument for why ontologists should be taken to speak English, rather than other ontological languages, comes from considering the relevance of anti-individualism to language-switches.<sup>92</sup> Tyler Burge has influentially argued that the content of mental states, and linguistic meaning, is not only determined by an individual's intrinsic properties and dispositions, but also by the other speakers of the linguistic community. The meaning of language is to some degree dependent on the social environment. Anti-individualism has bearing on the plausibility of ontologists speaking different languages because if we assume the truth of anti-individualism (in some form or other), then it in effect becomes harder for an individual of the linguistic community to "drift away" from the common language. For if a member of a linguistic community uses the language in an unconventional way, then instead of simply assuming that she is speaking her "own language", we must consider whether she is having a false belief, because the content of thought, and meaning of language, is in part determined by the linguistic community. So if an ontologist born in England starts using quantificational phrases (and consequently other words) in a, relative to the overall linguistic community, deviant way, then the fact that meaning is determined in part by her linguistic community creates inertia against us interpreting her as suddenly composing her own linguistic community with a language that is systematically different from English.

Hirsch is aware of the potential relevance of anti-individualism to his arguments (2011, p. 146–8; 229), but attempts to "circumvent" the topic, because he "think[s] it doesn't have any bearing on what I'm driving at" (2011, p. 229). He writes that to get around anti-individualism:

... I'm going to stipulate for the purposes of this discussion, the language of side X in any dispute is the language that would belong to an imagined linguistic community typical members of which exhibits linguistic behavior that is relevantly similar to X's. We can, if we wish, think of X as forming its own linguistic community. (Hirsch 2011, p. 229)

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<sup>92</sup> The paper that really introduces the idea in Burge (1979), but also Burge (1986) is a central work on anti-individualism. Burge (2007) comprises Burge's essays on anti-individualism.



In this quote, X is a stance with respect to an ontological question. What Hirsch in effect is doing, is creating a linguistic community by stipulation or imagination. As I explained in chapter 1, Hirsch allows himself to liberally define (imagine) languages in which certain deviant sentences, e.g. “A glass is a kind of cup”, or to take an example with bearing on ontology, “There are no tables”, are true. Important for the topic of this section is that this is done without regard of whether any actual philosopher can end up speaking this as a public language. Plausibly the Burge-point has no bearing on the claim that different ontological languages are possible (we can imagine them), but it *does* seem to bear on whether ontologists should be interpreted as actually speaking ontologically different languages.

The same disregard for this topic is found in another paper arguing for (iv) (Hirsch 2005). When trying to formulate what it takes for there to be a verbal dispute between himself and a speaker, A, who believes that a glass is a kind of cup, Hirsch writes:

What is important for my purposes is that the sentences asserted by A are true in A-English, so that the only real question is whether A-English is plain English. This is why the dispute with A is merely verbal. In effect I am redefining “A’s idiolect” to mean the (imagined) public language associated with A’s position. This redefined sense of “A’s idiolect” captures the relevant sense in which “A is right in A’s idiolect (and we are right in ours). (Hirsch 2011, p. 148)

Clearly, an idiolect can have idiosyncrasies, but Burge’s point is that in some cases, linguistic idiosyncrasies don’t result in personal linguistic communities, but rather in false beliefs. Again, Hirsch is here primarily concerned with defining a sense in which a dispute can be verbal – one is correct in one’s own language – but doesn’t consider whether actual speakers can end up speaking these different languages.

Hirsch never engages with the question of how Burge’s point affects whether ontologists that start out speaking English can end up speaking artificial ontological languages. If we assume that Burge is right about anti-individualism, it becomes harder to believe that ontologists, commonly speakers of English, should be interpreted as speaking a distinct ontological language *merely because* they have unconventional beliefs about what there is. In general, disagreement with the overall population doesn’t create linguistic sub-communities. Anti-individualism generates inertia against ontologists ending up speaking ontological languages radically different from English. This is of course no absolute constraint on interpretation, but it seems to suggest that merely having unconventional views on what there is hardly suffices to detach from English and drift off into other languages. The alternative explanation of the philosophers’ disagreement with the folks is this: someone is simply wrong.

Considering these four arguments together, I believe that ontologists should be taken to speak plain English, which means they are not engaged in a verbal dispute.

### 3.3 Against the argument from charity

If ontologists are speaking English, (iii) says that considerations of charity imply that their non-commonsensical ontological claims are “trivial falsehoods”, and thus ontology is rather pointless. In this section, I argue that Hirsch’s application of the principle of charity when interpreting English is flawed, so (iii) is false. The contention is that the elements of charity Hirsch emphasizes don’t make it plausible that revisionary ontological claims are false in English. The goal is not to argue that any specific ontological conclusion is true in English, that is a job for first order ontology, but simply to keep it open which sentences expressing ontological claims are true by denying that such revisionary sentences are “trivially false”.<sup>93</sup>

Some big picture issues first. If ontology is possible, then only one ontological theory is correct. English sentences expressing this ontological theory are true; sentences contradicting it will be false. On the naïve interpretation of English that Hirsch thinks follows from a correct application of the principle of charity, the English sentence “There are tables” is true, so nihilism is false. If the English sentence “Abraham Lincoln had in 1860 a temporal part that was bearded” is true, then a perdurance-theory is true. Thus there is no “special class” of ontological assertions; they are simply everyday English quantificational sentences.<sup>94</sup> Whether this is ultimately a tenable theory of English is a question for another day. Here I simply ask whether Hirsch has shown it untenable by arguing that revisionary ontological claims are false.

Hirsch’s argument for (iii) is based on “correct linguistic interpretation”, cf. chapter 1. I will argue that an interpretation of English in which sentences about the existence of ordinary composite objects come out true is not interpretatively superior to one in which they come out false, so revisionary ontological claims are not trivially false. To do this successfully, I argue that ordinary people are correct in asserting sentences about ordinary objects. The discussion will focus on the debate over composition, because it is the simplest and easiest to frame.

The dispute between Hirsch and me is over whether the principle of charity entails the falsity of revisionary claims. Hirsch gives the following description of the principle of charity:

Suppose we have two candidate interpretations for a set of sentences that fluent speakers of a language would typically be prepared to assert (or assent to). If one of these interpretations im-

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<sup>93</sup> In the following, all I say about language and which sentences are true pertain to the natural language English. The reader should note that I now disregard the question of whether ontologists speak different languages, and only focus on whether there are good reasons to think that revisionary ontological claims are “trivially false” in English.

<sup>94</sup> This approach is in line with Merricks (2001, chapter 7), but in contrast to some other important suggestions on the semantics of ontological claims. For instance, van Inwagen (1990, 2014) believes that there is a special “ontological context”, and that utterances of ontology doesn’t really contradict ordinary utterances. Sider (2011) believes that ontology (sometimes) is, or should be, conducted in a special ontological language, see chapter 4.

plies that the speakers are correct in asserting these sentences, and the other interpretation implies that they are incorrect, then the principle of charity imposes a presumption in favor of the first interpretation. (Hirsch 2011, p. 98)

In this context, “correct” and “incorrect” should be taken at face value, and not to mean “true” and “false”. This seems to be Hirsch’s understanding of the principle, and it is the understanding the revisionist must have to avoid the argument. To see this, consider a revisionary interpretation, *RI*, and a Hirschian one, *HI*, for a set of ordinary English sentences. On the revisionary interpretation, *RI*, some ordinary ontological claims are false. On the Hirschian interpretation, *HI*, those claims are true (assume that these are the only differences between the interpretations). In that case, the principle of charity understood in terms of truth says that we must maximize truth, and thus choose *HI* over *RI*. However, if we take “correct” to not necessarily entail truth, there is room for the revisionist to argue that *RI* is as correct, or as good an interpretation, as *HI*, effectively undermining the argument from charity.

Hirsch believes that the principle of charity has “more to do with *rationality* – with *good reasons* – than with *truth*” (Hirsch 2011, p. 99). This seems to be generally accepted in the literature. The exact role the principle of charity is supposed to play in a complete metasemantic theory is difficult to pin down, as we are far from having such a theory, but to the extent that the principle of charity is justified in an assumption of rationality on behalf of the target of interpretation, it is unclear why charity should be a guide to *truth* – there are other ways to be rational than by having true beliefs. Because of the difficult foundational questions that arise in this area, my reply to Hirsch will focus on the specific arguments Hirsch formulates based on the principle of charity. Thus I will not attempt to analyze how correctness integrates with truth or rationality, but take an intuitive notion of correctness for granted. However I will broach the issue more as we get clearer on what exactly Hirsch’s arguments demand of *RI* for it to be as correct an interpretation as *HI*.

In light of this, we may say that the principle of charity imposes an explanatory demand upon the revisionist. It will be practical to have it clearly stated:

*Expl* Revisionary ontologists must explain how the assertions of ordinary people about ordinary objects are correct, albeit false.

Whether assertions of a certain kind are correct in the required sense must be argued on a case-by-case basis. The ontologist must explain how it is that ordinary people have the beliefs they have, when they are false. As far as I understand it, most comprehensive revisionary theo-

ries attempt to satisfy *Expl*, though in different ways.<sup>95</sup> What's important to note initially, is that if such a project can be carried out to satisfaction, there seems to be nothing in the principle of charity that blocks the revisionary ontologist's claims. That's not to say that there aren't *other* reasons to believe in composite objects. Surveying and replying to all such arguments is a tremendous task, however, and this chapter is dedicated to showing Hirsch wrong.

In the following, I assume that what physical objects there are is an objective matter. I will talk about this as there being "object facts", but this might simply be what is often called unstructured facts. It is important that this notion is understood in this minimally demanding sense, and not understood as saying anything about the world's structure, fundamentality, ground or other metaontological notions. This is an admissible assumption because the issue in the argument from charity is *not* whether there are such facts – if it were, this assumption would be begging the question – but whether "correct linguistic interpretation" of English implies that any non-commonsensical ontological theory of physical objects is "trivially false". This shouldn't be a controversial assumption, for as long as one isn't an anti-realist, holding that the facts about which objects there are obtain *because* we have the beliefs we have, there must be facts that determine which objects there are. Hirsch believes that there are such facts, because he believes that the sentence "There are tables" is objectively true in English.<sup>96</sup> That's all that's required.

Metaphysicians may have different opinions on the character of these facts (for instance whether they are structured or unstructured) and what the facts in fact are. The goal of answering such questions is outside the scope of the present thesis, and all I need to assume is that English sentences are true in virtue of the objective facts. This is simply realism.

I have said that the ontologist must explain how it is that ordinary people are correct although they have (wildly) false beliefs. I will not be able to take on all arguments for why ordinary people may end up having the beliefs they do. There may for instance be other explanatory virtues connected to having ordinary objects in one's ontology, but such arguments belong to the substantial debates that Hirsch wishes to *avoid* by the argument from charity. I

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<sup>95</sup> See, for instance, Merricks (2001, chapter 7) for an explanation of how belief in ordinary objects are "close to true"; Dorr (2002, p. 77–110) for a fictionalist account of ordinary talk about composite objects, and Chisholm (1976) for an account of how ordinary people are correct in a "loose" sense.

<sup>96</sup> Hirsch thinks such facts are unstructured, and that they can be described in a wide variety of ways. Thus how we *talk* about these facts will vary from language to language. This follows from Hirsch's combination of quantifier variantism and realism. Hirsch firmly believes that the sentence "There are tables" is objectively true in English, and that the sentence "It is not the case that Abraham Lincoln had a temporal part in 1860 that was bearded" is objectively true in English. By virtue of being objectively true, there are facts making the sentences true (2011, p. 72–79). On the other hand, if we were speakers of another ontological language, other ontological claims would be true, and thus the facts, as we describe them, would be different, but the unstructured facts wouldn't be. How we describe the "facts" is a language-sensitive matter, though *truth* isn't. See especially Hirsch (2002a) for more on this.

shall therefore restrict myself to whether considerations of charity implies that *RI* is inferior to *HI* because *RI* entails that ordinary people are incorrect, while *HI* does not.

### 3.3.1 Problems with charity to perception

Consider an empty room containing what we would ordinarily describe as a table and a chair. Everyone agrees that there are particles arranged tablewise and that there are particles arranged chairwise in the room. The ontological question is what more, if anything, these things compose. The English-speaking nihilist says “nothing”, the ordinary English speaker says “a chair and a table”,<sup>97</sup> while the English universalist says “a chair, a table, a ‘table-chair’”, and various other sums of the particles. What reasons do they have for their beliefs? Both the universalist and the nihilist can probably cite quite complicated philosophical arguments. The ordinary person claims to have a quite different, yet powerful, reason for her belief: her perceptual experience. For it looks as if there is a chair, a table, and no other composite objects in the room. The revisionary ontologists have to say that the ordinary person is mistaken when she claims that the correct perceptual report of what she sees is “The room only contains a table and a chair”. In doing so, they would be going against the strong presumption imposed by charity to perception, and say that the ordinary person is incorrect. Thus *HI* would be a better interpretation than *RI*, and the argument from charity creates trouble for the revisionist.

In none of his essays does Hirsch discuss charity to perception in much detail. He seems to take it as quite obvious that people’s perceptual reports are *true*. The closest Hirsch comes to arguing for why charity to perception should be given special weight, is to claim that there is a “very strong presumption that any language contains sentences used to make perceptual reports, and that these reports are generally accurate” (Hirsch 2011, p. 149). Hirsch takes it for granted that our beliefs in ordinary objects are supported by our perceptual experiences, but this doesn’t entail truth. Hirsch claims that because ontologists must view disagreeing folks as delivering mistaken perceptual reports, they violate charity to perception (2011, p. 153).

Let perceptual reports be sentences speakers of English are disposed to utter to describe which physical objects there are on a given occasion. As has been alluded to several times, it is quite clear that revisionary ontologists must hold that many of the perceptual reports of English speakers are false.<sup>98</sup>

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<sup>97</sup> I’m here ignoring the other objects she perhaps believes the room contains, such as the material the chair is made out of, the table’s legs, the particles that make up the objects, and so on. I do this for ease of presentation.

<sup>98</sup> This depends on what kind of revisionist ontologist one is. Nihilists must reject most ordinary assertions, while presumably universalists must reject quite few. This depends on how one considers what is said, though: When the ordinary person says that all she sees is a table and a chair, we might understand this as excluding that there are other composite objects apart from the parts of the table and the chair. If we understand the ordinary person thus, then the universalist also has to reject many assertions.

To sum this up, charity to perception is the presumption that an interpretation must let perceptual reports come out generally accurate. Perceptual reports being true is one salient explanation of why they are correct, but it's not the only one. Hirsch writes that:

Revisionists have no plausible way of explaining why people make the mistakes revisionists allege. According to (quasi-) nihilists, for example, people mistakenly judge tables to be in front of them when there are no tables in front of them. (Hirsch 2011, p. 113)

Note that this denies that the revisionist can satisfy *Expl* with regard to perceptual reports. The challenge is this: The revisionist must explain how it can be that ordinary people are correct (don't make mistakes) when giving perceptual reports to the effect of existence of composites.

This challenge can be met. For the revisionist can argue that we have the perceptual experiences and beliefs as of composite objects merely because of biological and cultural contingencies, and not because of what there is; there is no sufficient explanatory connection between our beliefs about what there is and the facts about what there is.<sup>99</sup> For on the assumption of realism, our perceptual experiences must somehow be a function of the object facts if we are to give them any ultimate epistemic value when determining what there is. In the absence of such an explanatory connection, our perceptual experiences about which composite objects exist are ultimately undermined as truth-conducive. On the other hand, that doesn't mean ordinary people don't have good reasons for relying on their perceptual experiences when forming beliefs. As I argue below, the revisionist *can* say that perceptual reports are generally accurate without being true.<sup>100</sup>

I have provided the gist of what the revisionary ontologist would say about perceptual experiences as a guide to what there is. The relevance to the argument from charity is that if perception ultimately does not provide justification for believing in ordinary objects, then if the revisionist can explain that perceptual reports are generally accurate, *HI* and *RI* would be equally good interpretations. I will now consider details and objections.

A natural objection is that it *appears as if* there are tables and chairs, and that in the language we speak – English – it is correct to describe certain situations as containing tables and chairs. There is no doubt that it *looks as if* there are tables and chairs, and that our language naturally carves reality up in such things, but these data points are the result of biological and cultural contingencies that undermines them as truth-conducive. Our perceptual

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<sup>99</sup> Compare Korman (2014). I discovered Korman's recent paper in the process of working on this section. It is the only paper I know of that considers these topics in any detail. Korman (2014, p. 2) writes that such arguments haven't been examined in any detail. His paper gives a good presentation of the argument, and generally presents a good overview of this type of argument, so I will to some extent rely on his paper in this discussion.

<sup>100</sup> For a detailed framing of the argument, with thoughts on whom it is a problem for and the relationship between the explanation-formulation I choose and a sensitivity-version of the argument, see Korman (2014, p. 3–6).

system has contingent features that privilege “seeing something” only if the parts that make it up are related in a special way.<sup>101</sup> When we recognize this, we recognize that our perceptual experiences don’t give us reason to believe that exactly *those things* exist. This doesn’t mean that ordinary people don’t have good reasons for believing what they do, *inter alia* because they don’t have the defeater I’m now presenting. Relying on one’s perceptual system is *prima facie* reasonable (more on this below). Perceptual reports are based on perceptual experiences, so if perceptual experiences aren’t guides to what composite objects there are, we have little reason to consider perceptual reports true for the sole reason *that they are* perceptual reports.

Recall that everyone agrees that in the room described above, there are particles arranged chairwise and tablewise. More generally, they agree on the distribution of fundamental particles and the laws of physics. The ontological question is whether there are more objects in the room. Our answer is that perception won’t tell us, because perceptual systems aren’t sensitive to which composite objects, if any, there are. As Cian Dorr puts it:

If your visual system is functioning normally, your visual experience depends only on the motions of the photons in the vicinity of your eyes. And it seems to be a consequence of any remotely acceptable theory about photons that the behaviour of photons depends entirely on the arrangement of small things: what sorts of elementary particles there are, and how they stand to one another; what the curvature is at each point in spacetime; that sort of thing. Nowhere in physics do we come across a law that predicts that a photon will behave one way when it comes across some particles that compose something, and behave a different way when it comes across some particles of the same sorts, arranged in the same ways, that don’t compose anything. Similar claims seem to be true for the other sense modalities. Hence, whenever some things in your environment compose something, things would have looked, sounded, felt, smelt and tasted to you exactly the same way that they actually do if those things had been arranged exactly as they actually are, without composing anything. (Dorr 2002, p. 26)

The same point applies if there are more composite objects than ordinary people recognize, e.g. arbitrary sums or temporal parts. Thus the object facts themselves seem immaterial in determining one’s perceptual experience. On the other hand, the *particle facts* are essential to determine perceptual experiences. This explains the sense in which ordinary people are *correct* about what there is: given the intimate relationship between a composite objects and its parts (they are at exactly the same location, the particles collectively weigh the same as the composite, and so on),<sup>102</sup> we can explain that ordinary people form beliefs that is a function of the

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<sup>101</sup> We should distinguish the biological and the cultural contingencies relevant for explaining why we have the beliefs we have in (1) perception and in (2) language/thought. One question concerns to what extent our evolutionary developed perceptual system privileges certain distributions of light and edges and present “objects grouped up” to us. Another is how one’s personal environment affects how the perceptual system functions. A quite different question is how culture and other factors may influence which things we consider objects for other reasons than “seeing them as one”. For instance, we may have practical reasons to talk about some particles as one object. The reason for not going deeper into these questions is that they do not affect the central point: the truth of perceptual reports are not supported by any obvious data from perception or language.

<sup>102</sup> For a more thorough treatment of the relationship, see Sider (2007) and in general the literature on the composition as identity.

particle-facts. The way our perceptual system is set up, we experience only some particle-arrangements as a thing, but our experiences as of tables and chairs, but not table-chairs, appears to be a biological contingency without any explanatory relationship to the object facts.

One could object to this line of argument and say that our perceptual systems have evolved to fit the object facts, and thus that our perceptual experiences do stand in an explanatory relationship to them. Our perceptual experiences are the result of the sophisticated processing of the light registered on our retinas by our perceptual systems, and the perceptual system has evolved to be sensitive to the object facts.<sup>103</sup> This is not the claim that the objects are there because our perceptual system says so, but rather that our perceptual system presents the objects as there because it has evolved to recognize them.

There are two responses to this argument. The first is that evolution is not a process that always leads to truth; it is a process that leads to organisms having certain properties (the fit ones), but those don't have to be related to truth in a one-to-one fashion.<sup>104</sup> So for instance, the claim that perception has ended up matching reality in the sense that all ordinary objects, and no arbitrary sums of those, exist, is rather implausible. Hawthorne is onto the same idea:

Barring a kind of anti-realism that none of us should tolerate, wouldn't it be remarkable if the lines of reality matched the lines that we have words for? The simplest exercises of sociological imagination ought to convince us that the assumption of such harmony is altogether untoward, since such exercises convince us that it is something of a biological and/or cultural accident that we draw the lines that we do. (Hawthorne 2006, p. 109)

One would expect the crooked path of evolution to create some divergences between fact on the one side, and perceptual experiences and how we talk about them on the other.<sup>105</sup>

Furthermore, it would seem that the object facts have no role in explaining what kind of perceptual experiences are adaptive (Korman 2014, p. 2). Assume that in the situation described above, there are no tables or chairs, but rather an upthair and a downthair, that is, one object composed of the upper half of the atoms arranged tablewise and chairwise, and another object composed of the lower half of the atoms arranged tablewise and chairwise. Given such a situation, we wouldn't be at an evolutionary disadvantage compared to someone who con-

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<sup>103</sup> Our perceptual system is magnificently sophisticated. The light that hit our retina is only registered in two dimensions, even though they come from a three-dimensional space, so there is an underdetermination of information. The visual system has certain transformation mechanisms that incorporate information of how light usually is reflected from particulars and to the sensory registration of the retina, and uses these transformations to make generally accurate perceptual states. Visual illusions are examples of cases where our visual system is "tricked" to represent a situation inaccurately. See Burge (2010) for much more on this.

<sup>104</sup> See Burge (2010, pp. 292–308) for a thorough criticism of "naturalistic" meaning theories that attempt to analyze truth and representation in non-semantic terms.

<sup>105</sup> This is *not* to say that the process is random. Independently of whether there are composite objects or not, the fundamental particles that make up these composite objects are there, and their distribution and interaction with photons have contributed essentially to how the evolution of the perceptual system of humans (along with more random elements of evolution such as fit relative to other organisms and so on).



ceives of it as a situation with an upthair and downthair, for we can respond to the environment in the same way. Such examples can be reiterated for temporal parts.

I've been asking for an explanatory relationship between the object facts and our perceptual experiences to ground the supposition of Hirsch that we must interpret ordinary people's perceptual reports as true. Can't one object that an ordinary person's belief that there is a chair and a table in the room is true because these objects *cause* her to have an experience as of a table and a chair? That is, the explanation of our beliefs in terms of the objects facts is simply causation.<sup>106</sup> Because the objects cause the perceptual states, we must interpret ordinary people as speaking truthfully when they talk about ordinary objects.

As we have seen, it is not the object facts themselves that interact with us. Photons interact with the particles of the alleged ordinary objects, but the ordinary object isn't identical with the particles, so we don't causally interact with them.<sup>107</sup> Rather, the information from the environment is processed by our perceptual systems, formed as they are by our evolutionary history and cultural contingencies. Faced with the recognition that the photons meeting our retina would be the same whether there are chairs or upthairs, the causal relation between the objects sending off light and the perceptual state presenting chairs and tables is not sufficient to vindicate the claim that there are chairs "out there", as opposed upthairs and downthairs.<sup>108</sup>

The scope of the conclusion of this argument must not be misunderstood: most of the facts about what there is would be untouched by the argument, such as there being some particles arranged spherewise there, some particles reflecting light with wavelength 500-550 nanometers over here, and so on. The causal relationship between such facts and our perceptual experience explains why we have the perceptual experiences we do on a given occasion. In contrast, if there are tables, this fact would seem causally inert.

Hirsch's strategy for answering arguments that threaten common sense ontology is to say that they are based on premises that must be false in English, because the sheer number of sentences contradicting revisionary claims far outweighs the number of sentences with revisionary consequences (see section 3.4). Regardless of the merits of such an argument, my argument of this section cannot be avoided in this way. For while revisionary arguments are part of ontology, the argument of this section is an epistemological "debunking argument" with counterparts in *inter alia* metaethics and the philosophy of mathematics. So for instance, Benacerraf (1965) famously argued that as we don't stand in a causal relationship to mathematical entities, it is difficult to see how we have knowledge of mathematical objects if Platonism

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<sup>106</sup> For philosophers who suppose, but do not argue, that causal connections between ordinary objects and belief in ordinary objects are sufficient to answer my argument, see Joyce (2006, p. 182) and Schetchter (2010, p. 438).

<sup>107</sup> This is if one doesn't accept the thesis called composition as identity, see Cotnoir and Baxter (2014).

<sup>108</sup> This kind of causal link is sometimes said to be deviant. See Peacocke (1979, p. 128) on deviant causal links.

is true. In metaethics, Joyce (2006) among others have argued against moral realism by pointing out that our moral beliefs are likely results of evolution, and that it's hard to see how we have knowledge of moral facts. Such arguments are structurally similar to my argument: they point out an explanatory disconnect between the alleged facts and our beliefs. I take it that Hirsch doesn't reject this kind of argument out of hand, and thus he must either come up with some response to my argument, or accept that perceptual experiences don't give justification for believing in ordinary objects, so there is no pressure from charity to interpret perceptual reports as *true*. That they are correct, or "generally accurate", is enough.

I have argued that perception does not provide justification for believing in ordinary objects. But to successfully show that *HI* and *RI* are equally good interpretations, and thus undermine the argument from charity, something more must be said about how it is that ordinary people's perceptual reports are correct. First, because it looks *as if* there are tables and chairs, but no table-chairs, it is not surprising that people rely on their perceptual systems and culturally entrenched languages to report what they see. But, as I have argued, how things appear is not, at the end of the day, a guide to which physical objects there are over and above particles. The folk's lacking interest in philosophy explains why they keep relying on perceptual experiences and thus are wrong. Second, people are *correct* because perceptual experiences do depend to a considerable degree on what there is. There is a determinate relationship between the fundamental particles, photons and our perceptual systems' processing of the photons meeting the retina. Third, the object facts are largely irrelevant to how we orientate and interact with reality. Most ontologists agree that ontological theories don't have direct empirical consequences, so it is not surprising that ordinary people haven't noticed the potential falsity of their beliefs. But this doesn't make the folks incorrect or irrational, rather the folks are quite rational when they, absent an undercutting defeater, rely on their perceptual systems and language to talk about which things there are. Their beliefs are close to true.<sup>109</sup> In a sense, their beliefs are *correct*.

Lastly, and fortunately, Hirsch has to a large extent provided the resources to explain this notion of correctness for us already. In chapter 1, I showed how Hirsch construed the truth-conditions of a perdurantist language in an endurantists language, and vice versa, and the same for a universalist and common-sense view on composition. The same definitions can be used to define what we may call the correctness-conditions of the ordinary ontological assertions of ordinary people.<sup>110</sup> Thus a universalist can construe the correctness-conditions of

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<sup>109</sup> For an explication of this notion, and a description of the "belief and practice" of ordinary people in light of quasi-nihilism, see Merricks (2001, chapter 7).

<sup>110</sup> See Chalmers (2009) for a related notion.

ordinary people by restricting the quantifier, for example. But correctness is not truth, and thus it is open to ontological debate what there is.

Charity to perception was above described as the very strong presumption “that any language contains sentences used to make perceptual reports, and that these reports are generally accurate” (Hirsch 2011, p. 149). As we have seen, the revisionary ontologist can accommodate this. Our perceptual reports are generally accurate in the sense that they reflect the distributions of particles, and they are correct in the sense that they work for the purposes of ordinary people. Because there is no explanatory connection between the object facts and our beliefs about what there is, Hirsch cannot claim to have an interpretation that lets the folk come out more correct or rational than the revisionist, so there is not pressure to choose HI over RI. This is what’s required to undermine Hirsch’s argument from charity to perception.

### **3.3.2 Problems with charity to understanding**

A second element of the principle of charity that was mentioned in section 1.4.2 was charity to understanding. Hirsch claims that there is a presumption that fluent speakers of a language have a “sufficiently adequate grasp of their linguistic and conceptual resources so that they don’t generally make a priori conceptually false assertions, especially when these assertions seem to be relatively simple” (Hirsch 2011, p. 149). This is supposed to be a problem for the revisionary ontologist because she holds that questions of ontology are a priori knowable, and thus is forced to concede that ordinary people make a priori mistakes. When a nihilist claims that the sentence “There is at least one building in New York City” is false, a sentence ordinary speakers consider true, she violates charity to understanding because she by implication says that ordinary people are wrong about something that is a priori knowable. According to this line of argument, it is implausible that fluent English speakers make a priori conceptual mistakes, so there is a presumption against an interpretation of English that implies that.

It is not clear that charity to understanding always imposes a strong pressure to interpret people in conformity to the a priori conceptual truths. Mathematics and logic are considered a priori disciplines that discover necessary truths, but there isn’t thereby pressure to interpret individuals to be correct about the more complicated aspects of mathematics and logic. For instance, the fact that the independence of the axiom of choice and the continuum hypothesis of Zermelo–Frankel set theory is a priori knowable does not create pressure to interpret people in conformity to these truths. Hirsch takes this into account, and claims that charity to understanding is important when the assertions seem to be relatively simple, “not ostensibly involving any complicated calculations or computations (2011, p. 149). So it would seem that it is not the a priority in itself that matters, but rather a certain obviousness, or

“simplicity”, ordinary people associate with sentences such as “There is at least one building in New York City”.

Hirsch’s main point seems to be, again, that the revisionary ontologist doesn’t have a good explanation of why ordinary people would make such mistakes:

If revisionists interpret the ontological assertions of common sense as a priori necessarily false then, assuming there are other available interpretations, this does *prima facie* violate the principle of charity, since people are not normally thought to have good reasons to assert what is a priori necessarily false. (Hirsch 2011, p. 100)

Thus again the question becomes whether ordinary people have good reasons for making a priori necessarily mistaken ontological assertions, which is just another instantiation of *Expl.* In contrast to attempts to explain how ordinary people are rational and correct in spite of having false beliefs, the specific question of why they can make a priori knowable mistakes hasn’t, as far as I know, been given much attention in the literature.

It would seem the obviousness of ordinary ontological claims disappears when we consider the conclusions of section 3.3.1. I argued that even though our perceptual experiences in the end don’t give reason to believe in tables, that doesn’t mean that it is incorrect to utter “There are tables” when faced with particles arranged tablewise. To some extent, this contributes to undermine the obviousness of ordinary ontological claims, for this obviousness is presumably in part based on the truth of perceptual reports. Hirsch holds that the truth-conditions of the English sentence “There are tables” are what we would otherwise characterize as there being particles arranged tablewise. But having removed the perceptual justification for such truth-conditions, it seems that it is utterly *unobvious* – though perhaps still a priori – whether “There are tables” is true. If we undermine the obviousness associated with sentences about ordinary objects, we also remove some of the pressure from charity to understanding.

One may also take issue with how Hirsch construes the “a priori”, and based on it derives a kind of obviousness that we must respect in interpretation. Hawthorne (2009, p. 217) argues that many real-life ontologists don’t fit Hirsch’s “profile”, i.e. his assumption that they treat ontological questions as being answerable by a priori reasoning, and writes that:

Even if they [ontologists] regard their favored ontology as necessary, they often do not presume any special a priori access to its truth. Being content rather to defend it on the grounds of broad theoretical virtues like simplicity, reasonable conformity with common sense, elegance, and so on. They thus regard such theses as that the physical facts fix the phenomenal facts, that there is some elite stock of fundamental properties and that classical mereology is correct as quasi-empirical thesis whose tenuous connection to experience is not different in kind to that of various bits of high-level physical theory. (David Lewis is a good example.) (Hawthorne 2009, p. 217)

Hawthorne's argument can be construed in two ways. One take on it is that it rejects all together that ontological truths are a priori discernable: a priori reasoning is not sufficient to determine what there is. Another way to construe it is that mainstream ontology involves a whole battery of a priori considerations that must be weighed against each other, such as conformity to common sense, elegance, integration of other theories, simplicity, and so on. The first construal would undercut charity to understanding playing a role, for if sound, it implies that people aren't making a priori mistakes. But also the second version will undermine Hirsch's application of charity to understanding, for it makes the answers of ontology unobvious (this holds for the first construal of the argument as well). It is unclear why the a priori of ontological questions should be given special attention in interpretation. Tracking back to Hirsch's explanatory challenge, we can explain that ordinary people make a priori mistakes because the questions require sophisticated a priori reasoning to answer. Ordinary people have gone through no such reasoning, reasonably relying on their perceptual experiences and culturally entrenched languages to express what there is. Thus there is no pressure from charity to understanding to choose an interpretation that treats ordinary people's utterances as true, because they *do* have good reasons for their mistakes, cf. *Expl*.

In general, the revisionist ontologist would want to say that answering ontological questions is so difficult that there is no significant irrationality associated with any position *per se* (though there might of course be irrationality on parties to the dispute if they fail to react rationally to the arguments). In any case, we can hardly attribute problematic levels of irrationality as long as a party with a position on the matter, e.g. an ordinary person who believes that there are tables, hasn't been faced with an argument to the effect that their commonsense ontology is false, or at least not obviously true. Charity to understanding hardly has a role to play in determining how to interpret English ontological claims, for incorrectness doesn't follow from being wrong about ontological questions.

### **3.3.3 Problems with charity to retraction**

Finally, consider the element of the principle of charity that Hirsch calls charity to retraction. As I explained in subsections 1.4.2 and 3.2.2, this is first and foremost an element of charity that may block a given interpretation as charitable if it doesn't take into consideration the potential change in assertions and belief in the face of new evidence.

As such, it would seem that charity to retraction could only benefit the revisionary ontologist. For she believes that ordinary people are wrong in many of their assertions about physical objects. Treating it as a live possibility that she is correct, we cannot exclude the possibility of ordinary English speakers eventually being persuaded and retract their ordinary

ontological assertions. Charity to retraction would thus, in an interpretation of English, imply that we should choose an interpretation conforming to the true ontological theory.

On the other hand, it not obvious that any revisionary ontology is true, let alone which one. Instead, Hirsch could be correct that what exists is exactly what we ordinarily talk about there being. Thus the most neutral view on charity to retraction is that it doesn't impose any constraints or presumptions when interpreting English. It doesn't create any pressure towards interpreting commonsense ontological utterances as true, but it doesn't create any pressure to interpreting them as false either.

One could imagine Hirsch trying to impose a converse retraction-consideration against revisionary ontology: Because ordinary people are *not* disposed to retract their common sense ontological assertions, there is a presumption to interpret those sentences as true. It should be noted that this is not a standard application of charity to retraction. It doesn't say we shouldn't interpret in a certain way because that interpretation doesn't respects how ordinary people would alter their assertions about what there is in face of additional evidence, but rather that absence of retraction creates a presumption that there is no more relevant evidence. Thus this reverse argument doesn't have the same standing as an argument based on the presumption of charity to retraction, but we need not take a stance on the difficult question of the general plausibility of this reverse retraction presumption since the argument based on it can be answered. Not surprisingly, the following bears resemblances to subsection 3.2.2 above.

Observe that the argument put forward on behalf of Hirsch presupposes the empirical claim that ordinary people *won't* retract their assertions in the face of new philosophical evidence indicating that their belief in ordinary objects are wrong.<sup>111</sup> As a matter of fact, both Hirsch and Sider have appealed to the dispositions of English speakers in an apparent attempt say something about this argument. Hirsch reports that when he taught a metaphysics course and tried to explain four-dimensionalism, a linguistics student “had a seizure of uncontrollable laughter” and told him afterwards that “she thought that four-dimensionalism was an absolute hilarious aberration of the English language” (Hirsch 2011, p. 128). Ted Sider replies:

My students' resistance to such sentences fades when they come to appreciate the subtleties of quantifier domain restriction. (Compare initial resistance to “there are six tables in the room” said of a room with two large tables, each made up of two smaller ones.) Resistance fades further when students master spatiotemporal thinking. Resistance (of the uninitiated — not of Hirsch!) is partially due to failure to grasp the proposed nature of strange objects. After a bit of innocent coaching, students see the analogy between strange and commonsense objects, and no longer finds the former linguistically preposterous. In my experience, only philosophers put up enlightened resistance. (Sider 2004, p. 68o)

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<sup>111</sup> As a matter of fact, Hirsch seems to believe this, see section 3.4.

It should go without saying that these kinds of first-hand experiences bear little evidential weight with regard to how ordinary people<sup>112</sup> in general would react to philosophical evidence. I would prefer some empirical data on this, but without any such, I will argue that this reverse charity to retraction argument would overgeneralize in an implausible way.

It seems clear that in the same way as philosophers don't evaluate the evidence and arguments concerning a philosophical topic alike, it is implausible that all "ordinary" people would be disposed to evaluate the evidence and arguments uniformly. This is so in ontology, but also in other areas of philosophy there is a perhaps surprising amount of disagreement.<sup>113</sup> I doubt there is much reason to believe that ontological arguments are generally less persuasive than for instance arguments trying to establish an epistemological or ethical conclusion. If we want to speculate about how ordinary people will react to ontological arguments for a revisionary conclusion, it would seem that a reasonable assumption is that they won't be more or less disposed to retract their ordinary utterances about ordinary objects than they are to retract their knowledge-assertions in the face of skeptical arguments, or their ethical assertions in the face of ethical anti-realist arguments. If this were the case, it would seem that a reverse charity to retraction presumption would generalize to other arguments in philosophy in an unwanted and implausible way. I find it unreasonable to think that skepticism is false merely because people are disposed to reject skeptical arguments, and by analogy, we should not reject an interpretation of English simply because it goes against "common sense".<sup>114</sup>

### **3.3.4 The argument from charity summarized**

In the previous subsections I have discussed the merit of the argument from charity. I argued that the argument from charity should be understood as the explanatory challenge to the revisionist to explain how the assertions of ordinary people about ordinary objects are correct, albeit false. To answer the argument from charity, the revisionist has to show that a revisionary interpretation of ordinary people's assertions, RI, lets them come out as correct as a Hirschian interpretation, HI. I have provided arguments for why ordinary people are correct when they take there to be ordinary objects and no arbitrary sums thereof.

I considered three distinct elements of charity. Charity to perception asks for an explanation of why ordinary people believe that there are ordinary objects, and I argued that upon reflection, there is little reason to believe that we have direct perceptual evidence for taking

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<sup>112</sup> Perhaps neither linguistics students nor Sider's students are "ordinary people", but in that case their reactions are even less relevant to the general question of how ordinary people will react.

<sup>113</sup> See the empirical studies published in Bourget and Chalmers (2014). For a direct discussion of the widespread disagreement, and lack of consensus in philosophy, see Chalmers (*forthcoming*).

<sup>114</sup> That's not to say that some may consider the conclusions of such arguments as so preposterous that they won't believe them and thus are motivated to find arguments against them. But as far as I'm concerned, this says nothing about the soundness of the skeptical arguments.

there to be ordinary objects (section 3.3.1). Nonetheless, the folks are correct in trusting their perceptual systems, since there is an intimate relationship between what there is and perceptual experiences; their beliefs are formed based on perceptual experiences that are the function of the fundamental particles, their beliefs work for their practical purposes and they don't have defeaters. I also argued that the a priority of ontological theories imposes no presumption to interpret English sentences about ordinary objects to be true, because such questions are very difficult to answer, so the a priority of the answers doesn't impose any significant incorrectness on ordinary people if they happen to be wrong (section 3.3.2). Finally, a converse charity to retraction-argument has little bearing on whether English sentences such as "There are tables" are true (section 3.3.3). In conclusion, the argument from charity does not deliver the conclusion that ordinary claims about what there is, such as "There are tables" and "There is no object that has Plato's nose and the Eiffel Tower as parts" are true. Thus it is open for the revisionist, as long as she is ready to meet the explanatory challenges I've discussed, to argue that revisionary ontological claims are true in English.

As I said at the outset, the purpose of this section was not to argue for a specific ontological conclusion, but simply to argue that the argument from charity does not entail that English sentences about ordinary objects are true. That's what I've done.

### 3.4 The Outweighing Argument

What will Hirsch say to my arguments? To those who claim that sentences such as "There are tables" and "There is no object that has the Eiffel tower and Plato's nose as parts" are *not* trivially true in English, Hirsch's reply has been as follows: The apparently true sentences that make out the premises of the argument against common sense are far outweighed by the number of apparently true sentence that *supports* common sense. So the premises must, correctly interpreted using the principle of charity, be false. Thus my argument is unsound.

This is the response Hirsch gives to David Lewis' (1986, p. 202–204) argument from the nature of temporal intrinsics to the doctrine of temporal parts. Hirsch writes that:

For it [Lewis' argument] is hardly likely to have the power to convert typical English speakers. Surely the typical speaker's disposition to talk like an endurantist far outweighs any disposition to worry about temporal intrinsics. So plain English remains E-English [a language in which the typical assertions of endurantists are true]. Insofar as the philosophers involved in the dispute claim to be speaking plain English, that's all they need to know. (Hirsch 2011, p. 231)

This argument is quite similar to the reverse charity to retraction-argument discussed in section 3.3.3. Hirsch talks about "dispositions" *outweighing* others. If a set of dispositions to talk a certain way outweighs another set of dispositions, then that's all we "need to know" about which language we are speaking. English speakers are disposed to consider "There are tables"



as true, so Hirsch's common sense ontology follows, entailing the falsity of revisionary ontological claims, and also entailing the falsity of the sentences (premises) in the arguments that have revisionary conclusions, for they are "outweighed". Here is another example:

But if the truth of the philosophical sentence conflicts with endurantism, as Lewis claims, then this sentence is false in E-English. The overwhelming considerations of charity to use that indicate that the language of our community is E-English – the charity to use that appeals to the typical speakers' confident assertions of innumerable endurantist sentences – must be weighed against charity to the philosophical sentence. This seems to be no contest. (Hirsch 2011, p. 232)

So Hirsch seems to focus on maximizing the number of sentences a linguistic community as a whole accepts as true when determining the truth of other sentences, where we also pay attention to the confidence the speakers have in their truth. The arguments in favor of revisionary ontological theories are explained away by arguing that ordinary people are much more prone to accept sentence such as "Ordinary objects don't have temporal parts" than "Qualities such as shapes are not relations, but intrinsic properties". Hirsch (Hirsch 2011, p. 231) claims that "Surely the typical speaker's disposition to talk like an endurantist far outweighs any disposition to worry about temporary intrinsics", and finds this sufficient argument for endurantism, which, as I've said, implies that revisionary theories of persistence are false. (For another example of such an argument, see Hirsch 2011, p. 185.)

Hirsch seems to take the presently assumed truth of sentences as a given, and then evaluates sentences with non-commonsensical philosophical implications, *S*, against these. The number of sentences that are taken to be true in the community that implies  $\sim S$  will far outweigh *S* in number and confidence, so *S* must be false. We may then characterize the general method thus. Take a set of sentence, *s*, that are confidently assumed to be true in English, and then evaluate every sentence with potentially revisionist consequences, *S*, against this set. In most cases, because of the infrequent philosophical discovery of sentences that seem true that (in an argument) implies that common sense ontology is wrong, or not obviously true, the number of sentences and the degree of confidence in the sentences in *s* will far outweigh our confidence in *S*, so *S* must be false. Thus no revisionary ontological theory can be true in English.

Evaluating this general theory of truth-in-a-language would bring us too far away from the central topic of this thesis,<sup>115</sup> but we should again question Hirsch's insight into ordinary people's dispositions. Basically, Hirsch is predicting how English speakers will react to philosophical arguments. We should note that this isn't a prediction that entails that the English

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<sup>115</sup> A discussion of Hirsch's overall view of language would be interesting, but difficult, and I've not found the space for it. Suffice it to say that I find Hirsch's approach to "truth in a language" somewhat simplistic and implausible.

language can't change. Hirsch is surely open to the possibility of the meaning of our terms changing, so that it becomes true to talk about arbitrary sums or temporal parts in English. But he thinks that we need to remain responsible to the language we claim to be using, and if we understand English sentences, then it is true that there are tables (Hirsch 2011, p. 85). What Hirsch must reject is that there can be *rational grounds* for speakers of English to give up common sense ontology at the present time, which is just what the revisionist believes. So Hirsch predicts that given the meaning of our terms now, people won't have a rational basis for changing their beliefs and assertions about which physical objects there are.

This is a prediction I find dubious, but it should perhaps be recognized that one's predictions in this area are influenced by one's stand on the arguments against common sense ontology. I feel some pressure to at least reevaluate my ontological beliefs in the face of ontological arguments, and thus I expect that others will reconsider their beliefs as well. In any case, I think we should abstain from basing an argument on a prediction about ordinary people before we have some empirical data.

In addition to this, the simple fact that the number of common sense quantificational sentences outweighs the number of sentences with revisionist consequences cannot be the whole story of the truth of either theory. This is the point about charity to retraction, and as I argued in 3.3.3, charity to retraction doesn't create any interpretative pressure in this debate, because we don't know what the object facts are.

My objection to this argument is that if my argument of section 3.3.1 is sound, then ordinary English speakers should, after being faced with that argument, accept that they don't have a rational basis for believing in ordinary objects based on their perceptual experiences. The argument of section 3.3.1 is an argument that *in general* undermines the confidence in the sentences in *s*, because it shows that our reasons for believing in objects are not tracking which composites there are in an adequate way. We therefore have a systematic altering of the confidence in the sentences in *s*, alternatively a general retraction of them, in effect reducing the pressure from charity to interpret *S* as false. It would seem that the outweighing argument loses force when we can undercut the justification of the sentences that are *used* to outweigh the truth of the premises of the arguments undermining common sense ontology. In light of the argument of section 3.3.1, then, ordinary people should reevaluate or retract many of their utterances, in effect reducing the number of utterances supporting common sense ontology.

Importantly, the argument of section 3.3.1 is not an ontological argument, but an epistemological argument that undermines the reasons we have for our common sense assertions. If Hirsch also wants to undermine epistemological arguments that go against "common sense" by the outweighing argument, then I must rest my case given the space here. I think the revi-

sionary ontologist should be satisfied if she can show that substantive ontology is no worse off than other parts of philosophy.

Second, if the argument of section 3.3.1 is sound, people should consider it “additional evidence”. The argument seems to eliminate the most obvious grounds for believing in composites, and thus it creates a rational requirement to at least evaluate the truth of one’s beliefs and assertions about ordinary objects, with the potential requirement to also consider the more traditional ontological arguments about composites. If someone undercuts your evidence, it is only rational to start thinking of other grounds for and against the beliefs that evidence earlier was thought to support. Thus it would seem that if the argument in section 3.3.1 is sound, it opens up the dispute, where there are arguments that undercuts the other reasons one may have to believe in composite objects (Merricks 2001), or that suggest that there are many more objects than ordinary people believe (Sider 2001b).

In ending, I will suggest an explanation of Hirsch’s tendency to disregard revisionary theories. Hirsch sometimes frames his arguments as a question about which of his completely interpreted languages is English. As we saw in chapter 1, Hirsch’s arguments involve the stipulation of a wide variety of different ontological languages, where it follows from the stipulation of the meaning of the languages that certain target sentences, such as “There are no tables”, are true. But the purpose of Quinean ontology conducted in *any language* is to determine which quantificational sentences are true, and it appears Hirsch has underestimated the difficulty of this process. Hirsch usually defines a language, “Eli Hirsch”-English in (2011, p. 164), E-English in (2011, p. 231), in which a common sense ontological theory is true. Because EH-English is the language that’s most similar to what most people think is true in English, Hirsch concludes that EH-English must be English. What I hope to have shown in this chapter is that interpretation of English is much harder than such a crude comparison between English and fully interpreted stipulated ontological languages suggest, and that revisionary ontological claims aren’t “trivially false” in English. Perhaps Hirsch’s stipulations of different languages with already-defined truth-conditions have made him blind to the non-trivial task of determining the truth-values of natural language sentences. If this isn’t such a trivial task, as I’ve been arguing, perhaps there is room for ontology in English.

## 4 Normative Quantifier Invariantism

### 4.1 Ontology Needs a Plan B

Suppose that the arguments of chapter 2 and 3 fail. Weak quantifier variantism is true, and the argument from charity implies that revisionary ontological claims are false in English. Then there is no point for ontologists to conduct their inquiry in English, and choosing to do ontology in some other ontological language seems to imply an arbitrariness ontologists wish to avoid. Is all hope out for mainstream ontology? Perhaps not, maybe there is a “plan B” for ontology, as Ted Sider argues (2011, p. 74). This chapter examines Sider’s proposed rescue plan for ontology and evaluates whether it is up to the task of answering the challenge from quantifier variantism. There are other ways to argue for the substantivity of ontology,<sup>16</sup> but as Sider’s theory is developed in direct engagement with Hirsch’s arguments, adopts a Quinean view of ontology, and has received much attention lately, it is the most interesting theory to focus on in the context of this thesis. The fact that Hirsch thinks that the most significant response to his deflationary arguments are found in Sider’s work makes it all the more natural to look closer at Sider’s response (e.g. Hirsch 2011, p. 121; Hirsch 2013).

In chapter 2 I argued against Hirsch by targeting weak quantifier variantism. Sider, on the other hand, seems to accept the basic claim of weak quantifier variantism (2011, p. 74f; 172f).<sup>17</sup> His suggestion on behalf of mainstream ontology is to instead reject strong quantifier variantism, which was defined in chapter 1 as:

- (ii) The possible truth-conditionally equivalent languages are of equal metaphysical merit.

Given the definitions in chapter 1, rejecting (ii) is normative quantifier invariantism. It says that there is a best, or privileged, ontological language. Following Sider, we call this language – if there is any such – *Ontologese*. Different ontological languages are truth-conditionally equivalent, so the normative quantifier invariantists must hold that Ontologese is a metaphys-

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<sup>16</sup> Contemporary metaontology provides several frameworks for ontology. Some adopt the Quinean conception of ontology and attempts to avoid Hirsch (e.g. Dorr 2005, van Inwagen 2014), while others reject the Quinean picture and instead introduces distinctive ideology that are supposed to make room for substantive ontology, for instance Fine’s reality-operator (2001, 2009), and Schaffer’s notion of ground (2009). As these latter projects reject the Quinean picture underlying Hirsch’s arguments, they are unfit for discussion here. Place constraints inhibits me from discussing the other “Quinean” alternatives.

<sup>17</sup> Sider concedes that there are several quantifier meanings, so it would seem that he accepts weak quantifier variantism (Sider 2011, p. 74f; 172f). On the other hand, and as we will see in section 4.5, exactly how to characterize his reply to Hirsch is perhaps a little more complicated than what I’ve said indicates.

ically better language along some dimension that isn't truth; when talking about what there is, truth is not enough:

For a representation to be fully successful, truth is not enough; the representation must also use the right concepts, so that its conceptual structure matches reality's structure. There is an objectively correct way to "write the book of the world". (Sider 2011, p. vi)

Truth has traditionally been taken to be the ultimate goal of inquiry and belief.<sup>118</sup> What's distinct – and novel<sup>119</sup> – with Sider's account, is that it postulates on a new, *constitutive epistemic norm for inquiry* in virtue of which Ontologese is better than other ontological languages (2011, p. 61; 172). I will get back to the exact formulation of this norm after I've presented the basic tenets of Sider's theory, but we may already now note that this requires that we use the "right concepts" when describing reality. When talking about what there is, we must use the right quantifier, which Ontologese is defined to employ. The existence of an epistemic norm besides truth is what makes Ontologese privileged, and such a norm is the essential ingredient of normative quantifier variantism.

Sider (2011) offers a completely new framework for metaphysics, if not philosophy in general. The theory is complex, and has a wide range of applications and subtleties. To make the discussion of Sider's specific answer to the quantifier variantist manageable in the allotted space, I've chosen to focus quite narrowly on the justification of the novel epistemic norm he suggests and its relation to normative quantifier variantism. This meshes well with the dialectical emphasis on the distinction between strong and weak quantifier variantism, and is a topic that to my knowledge hasn't received much attention so far in the literature. This rules out some important connections between Sider's view and quantifier variantism in general, but I will briefly return to these in section 4.5. Thus this chapter will not be about quantification *per se*, but instead about whether there is a privileged quantificational description of reality.

The connections between Sider's general theory of structure and the epistemic norm that makes Ontologese privileged for ontology is somewhat intricate, so I will start with a general overview of Sider's theory in section 4.2. I describe the norm in 4.3, followed by section 4.4, which argues that such a norm is not well enough supported, which seems to undermine Sider's defense of ontology against quantifier variantism. However, section 4.5 points to some complications with characterizing Sider's view as normative quantifier invariantism, and sug-

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<sup>118</sup> Bernard Williams has said that truth is the "aim of belief" (1973, p. 148) and Searle has claimed that beliefs have mind-to-world direction of fit (1979). If they are correct, our conclusion follows. Whatever the nature of belief in and of itself, I take it as uncontroversial that the aim in metaphysics and ontology is (at least) truth.

<sup>119</sup> See Hirsch's (2013, p. 713) remarks about the differences between Sider's metametaphysics and the metametaphysics of other philosophers (among them Fine and Schaffer).

gests a way in which Sider's framework can be used to provide a response to Hirsch without relying on a new epistemic norm.

## 4.2 Structure

The theory that Sider's variant of normative quantifier invariantism is part of is not merely a metaontology. Sider (2011) puts forward a framework that is in the full sense a metametaphysics with consequences for a wide range of fields.<sup>120</sup> The goal of Sider (2011) is:

... to push forward the front of realism about structure. I want to expand our conception of structure's importance, generalize the concept of structure, investigate its nature, use it as the foundation of 'metametaphysics', and reconceptualize metaphysics in terms of it. (Sider 2011, p. 5)

The breadth of the project is vast. The central thesis of the theory is that "The world has a distinguished structure, a privileged description" (2011, p. vi). We may note initially that there are two claims being made here. One is the claim that the world has a distinguished structure, another the claim that there is a privileged description of this structure. It's this latter claim that is related to the epistemic norm; it is in virtue of the norm there is a privileged description. Nonetheless, the central notion is *structure*, which is a primitive notion and an ideological novelty defined by the theoretical role it plays (2011, p. 10).<sup>121</sup> The notion can be viewed as a generalization of David Lewis' notion of naturalness (*inter alia* Lewis 1983b, 1984 1986).<sup>122</sup> Before I move on to the notion itself, it will therefore be useful to say something about Lewis' original notion.

According to Lewis, some predicates "carve nature at the joints". The intuitive idea is that some predicates provide a better fit with objective reality than others (Bøhn 2014, p. 337). There are many different properties, but some properties are more natural than others (they constitute an *elite* class of properties); naturalness is a property of properties<sup>123</sup> and properties vary in naturalness (a property can be said to have a 'degree of naturalness'). The perfectly natural properties tell us what the world is like, fundamentally speaking, and constitutes the joints in nature. Lewis connects naturalness to objective similarity: natural properties go better together than less natural properties. A much-used example to illustrate the notion is that green is a more natural property than the property of being observed before *t* and being green,

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<sup>120</sup> The theory is broad in scope, developed to reconceptualize, substantiate and clarify debates about modality, causation, time, logic, and so on. One application of it is to ontology, and that's the one I will be concerned with. But it must be kept in mind that the basic notion of structure has an impact and application beyond ontology.

<sup>121</sup> I will not go into the number of topics Sider connects to structure, but they help explain the notion and show that it can be of use in a wide range of philosophical areas. The number of connections is also important for our grasp of the notion, as it is through these connections that we grasp it. See Sider (2011, chapter 2) on the epistemology of structure.

<sup>122</sup> Another predecessor of the notion is Armstrong's theory of universals (1978a,b).

<sup>123</sup> I use 'properties' here to include both monadic properties and relations.

or not observed before  $t$  and being blue (grue) (Goodman 1955). Green things are more similar than grue things; they mark a more natural joint in nature. Likewise, the property of being an electron is more natural than the property of being an electron or a cow. Electrons go better together than electrons and cows. Naturalness is, like structure, a posit understood and justified by the theoretical role it plays,<sup>124</sup> and it is supposed to, *inter alia*, make for objective similarity and classification (Lewis 1983b, 1986), solve Putnam’s paradox (Lewis 1984) and figure in natural laws (Lewis 1983b, 1986).

Whereas Lewis’ notion of naturalness only applies to properties, the central idea behind Sider’s structure is to extend the notion “beyond the predicate” (2011, chapter 6). Sider’s “structure” applies across linguistic categories – beyond the predicate – making it possible to query whether an expression of any grammatical category is structural (2011, pp. 91–92).<sup>125</sup> Let’s use “ $\text{struc}(\phi)$ ”<sup>126</sup> to say that the world contains  $\phi$ -structure and that “ $\phi$ ” is a structural term.<sup>127</sup>

Even though we say that it is concepts, or pieces of ideology, that are structural, structure is nonetheless worldly (Sider 2011, p. 5fn; 85f). This is analogous to naturalness. We ask whether a certain predicate – a concept, a piece of ideology – is joint-carving, and realize that by being joint-carving it expresses something objective about the world; the perfectly natural properties, the world’s joints. By extending structure beyond the predicate, also concepts that don’t predicate objects (such as tense operators and modal operators) can be structural and mark something objective about reality. Grammatical categories besides predicates can express something objective and fundamental about reality without predicating anything. For instance, quantifiers can’t be treated as predicates or relations,<sup>128</sup> so structure is “entity free” in contrast to naturalness (Sider 2011, p. 90). Thus the expression of “carving nature at the joints” becomes less apt, but Sider (and I) continue to use the predicate “joint-carving” to express that a certain linguistic item is structural.

Sider connects structure to fundamentality. The joint-carving notions are the fundamental notions (2011, p. vi). Details aside, discerning reality’s structure becomes a matter of determining which concepts are structural. An example: If tense-operators are joint-carving, then the world contains temporal structure, so tense is fundamental, and any comprehensive

<sup>124</sup> But it is perhaps different from structure in the sense that while structure is *only* defined and grasped in terms of its theoretical/inferential role, we also has a grasp of naturalness through its connection to similarity.

<sup>125</sup> So whereas only properties can be more or less natural, concepts of any grammatical category can be more or less structural. We can ask whether modal operators, temporal operators, quantifiers and even truth-functional connectives are structural. Deciding these questions amounts to discerning the fundamental structure of the world. I will use the expressions “concepts”, “ideology”, “expression”, etc. to talk about what may instantiate  $\phi$ . See section 4.3.

<sup>126</sup> Sider (2011, p. 91) uses a symbol where I use “ $\text{struc}$ ”, but this does of course not matter.

<sup>127</sup> I will, as Sider, slip into quantifying over structure. It is important to note that structure isn’t *a thing* in reality.

<sup>128</sup> One can treat some notions of interest as predicates and relations; negation and disjunction can be analyzed as standing for relations, for instance, but that would require propositions, and Sider wants to keep the question of the existence of propositions open (Sider 2011, p. 85).

description of reality must contain tensed vocabulary to be complete. Asking “Struc( $\phi$ )?” amounts to asking a question about whether the world contains  $\phi$ -structure.<sup>129</sup> Sider’s “reconceptualization” of metaphysics consists in transforming questions about the metaphysics of time, modality, ontology and so on into questions about whether the terms we use to express such notions are joint-carving. On the topic of metaphysics, Sider writes that:

The truly central question of metaphysics is that of what is most fundamental. So in my terms, we must ask which notions carve *perfectly* at the joints. By using ‘red’ and ‘blue’, we carve more closely to reality’s joints than do speakers of the ‘bred’/‘rue’ language. But we do not thereby carve perfectly at the joints; colors are presumably not perfectly fundamental. To carve perfectly, one must use the most fundamental concepts, expressing the facets of reality that underly the colors. (Sider 2011, p. 5)

In order for this reconceptualization to be successful, we have seen that several types of terms must be able to be structural, not just predicates. Because it is *ideology* that carve at the joints, we can talk about a fundamental language that uses only joint-carving notions. Given the epistemic norm mentioned above, “there is an objectively correct way to ‘write the book of the world’” (Sider, p. vii).<sup>130</sup>

The justification for believing in the general framework of structure is that it enhances our understanding of the world (Sider 2011, p. 9). The applications and interconnections of structure provide it with a rich theoretical role. Structure improves our theories and thus is a justified posit (Sider 2011, p. 10). This justifies the general framework of structure, but whether a certain structure obtains, for instance quantificational structure, is a matter of specific argument turning on whether it is plausible that a fundamental description of reality must include that specific notion. Sider (2011, pp. 180–189) has a lengthy discussion of the arguments for quantificational structure; quantification is indispensable in a complete description of fundamental reality. While I find those arguments persuasive and interesting, I will keep my focus on whether there is an epistemic norm to the effect that we must *describe* this structure, if it is there, using Ontologese.

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<sup>129</sup> Sider suggests a distinct way of deciding questions of how much structure the world contains, but a presentation and evaluation of this method is outside the scope of this thesis. See Sider (2011, p. 13f) for details.

<sup>130</sup> The question of which concepts are structural thus concerns how the world fundamentally is. As a theory about fundamentality, Sider’s view is that facts of fundamentality consist in facts of the form “T is structural”, where “T” is a sub-sentential part of language (see Fine 2013, p. 725). If the world contains *quantificational* structure, then there are some things in the fundamental sense of “there is”. If reality lacks modal structure, then modal facts are non-fundamental, and hold true merely in virtue of the fundamental facts (Sider 2011, chapter 12). Answering questions about which concepts are structural amounts to answering questions about how the world fundamentally is.



### 4.3 A New Epistemic Norm of Inquiry

What I've said hopefully gives a relatively clear picture of Sider's view.<sup>131</sup> What about his privileged description-claim, which must be kept apart from the structure-claim?<sup>132</sup> The different ontological languages are supposedly truth-conditionally equivalent, so Sider's defense of ontology is that "truth is not enough" – there is an epistemic norm besides truth to the effect that it is *better to think and speak in joint-carving terms* (2011, p. 61). Ontologese by definition employs a joint-carving quantifier, so on the assumption that there is quantificational structure, Ontologese is epistemically better than other ontological languages. In essence, this is Sider's response to quantifier variantism.

To continue the unpacking of the privileged description claim, consider what Sider writes:

The goal of inquiry is not merely to believe truly (or to know). Achieving the goal of inquiry requires that one's belief state reflect the world, which in addition to lack of error requires one to think of the world *in its terms*, to carve the world at its joints. Wielders of non-joint-carving concepts are worse inquirers. (Sider 2011, p. 61)

The representations of the structure, i.e. the language we use to describe the world,<sup>133</sup> must "use the right concepts, so that its conceptual structure matches reality's structure" (2011, p. vi). So there must be some kind of "match" between the concepts of a metaphysical theory and reality's structure for the theory to be fully successful.

Those who fail to use structural concepts when describing the world are making a rational mistake (Sider 2011, p. 2). They violate the epistemic norm Sider appeals to: it is *better* to think and speak in joint-carving terms. According to Sider, this is a "constitutive aim of the practice of forming beliefs, as constitutive as the more commonly recognized aim of truth" (2011, p. 61). I will call this the *s-norm*. The *s-norm* is not necessarily a conscious goal, but it is a standard by which beliefs and believers can be evaluated (Sider 2011, p. 61). This section describes the norm in more detail and the next section criticizes the arguments for it.

As we have seen, there is little doubt that if there is quantificational structure and an *s-norm*, then ontology is rehabilitated. But how are we to understand the norm that it is "better to think and speak in joint-carving terms"?

A first question concerns *when* this norm is operative. Considering that Sider thinks those who fail to conform to the norm are making rational mistakes, it would seem that the

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<sup>131</sup> That's not to say that it gives a comprehensive account of Sider's theory! The theory is brimming with applications and subtleties, but I've chosen to focus on the most important question for our purposes: whether Sider establishes that there is a privileged description of the world.

<sup>132</sup> That these claims are of different nature can be gleaned from the fact that while the structure-claim is a descriptive claim about the world, the privileged description-claim is a *normative* claim about inquirers.

<sup>133</sup> The language we use to "write the book of the world" in Sider's phrasing.

norm would imply too strong a demand on people if it were always operative. For instance, when knitting a sweater with a complicated pattern in different colors, it would seem too demanding to require that the knitter think of the colors in terms of photon wavelengths. Humans have limited cognitive resources, and in a wide range of practical situations where the goal isn't to discern reality's structure, it would be puzzling to say that people are making rational mistakes because they don't think and speak in joint-carving terms.

In the knitting-scenario, the goal of the process is to create a sweater, not to attain truth. But even in some cases where the goal is truth would it be odd to say that people are making a rational mistake merely because they aren't thinking in joint-carving terms. For instance, if I need to catch the last boat off some distant island, it is important that I know when the boat is leaving. If I use a time-indexical to do so, for instance "The boat leaves 50 minutes from now", and the world turns out not to contain temporal structure, my descriptive belief is not using joint-carving terms (because there are no joints for "now" to carve at). However, it would seem odd to say that I'm making a rational mistake merely because I expressed my belief in just this way, rather than by using eternalist vocabulary.

Sider himself is a little unclear about when he thinks the norm is operative. On the one hand, he writes that "we ought not to speak the 'grue' language", which seems to be a general statement about it being epistemically sub-optimal simpliciter to use non-joint-carving terms (Sider 2011, p. 61). But in most places, he seems to connect the norm to inquiry, and more specifically *fundamental* inquiry. Sider's book is sparse on reflection on the norm itself, but as mainstream ontology takes it as its goal to discern what the world is (fundamentally) like, it would seem that ontology, at least, qualifies as a type of inquiry in which the s-norm is operative.

A second question is what "better" means. It must mean that using joint-carving terms is simply epistemically better; we fulfill the goal of inquiry to a larger extent if we use joint-carving terms (see section 4.4 below). Thus if we fail to use joint-carving terms, but have true beliefs, we are making a rational mistake of some sort. Sider writes: "Ontological questions in Ontologese are substantive, even if those in ordinary language are not. Moreover, Ontologese is a better language, since its structure better matches reality's structure" (2011, p. 172). This is what makes Ontologese privileged; by using it, we use a joint-carving quantifier that matches reality's structure, and thus, when our quantificational claims are true, we achieve the full goal of inquiry.

A third question is what the exact content of the norm is. Sider formulates the s-norm both in terms of "concepts" (2011, p. vii; p. 2) and in terms of "terms" (2011, p. 19; 61f). I've chosen the formulations from the section "Epistemic value" (2011, p. 61f), where he argues for the

claim that it is better to think and speak in joint-carving *terms*, but Sider doesn't say what exactly he means by "term". To evaluate the norm, it is important to know the precise content of it.

Consider Sider's formulation of the structure-claim, " $\text{struc}(\phi)$ ", where " $\phi$ " is some term, concept, notion, or what have you, of any grammatical category. How are we to understand " $\phi$ " if the goal is to construe true structure-claims? One alternative is to let complex terms take the place of " $\phi$ ". In that case, structure-claims about disjunctive predicates could be true. So for instance, if we suppose that "green" expresses a perfectly natural property, then  $\text{struc}(\text{green})$  would be true, and  $\text{struc}(\text{grue})$  would be false. But the problem with such an understanding of  $\text{struc}(\phi)$  is that "green" is definable in terms of "grue" and "bleen", which are both non-structural terms themselves.<sup>134</sup> So this complex definition of "green" in terms of "grue" and "bleen", " $\phi$ ", would be truth-conditionally equivalent to "green", and thus if " $\text{struc}(\phi)$ " allows for complex expression, then any expression truth-conditionally equivalent to some structural primitive term can take the place of " $\phi$ ", and thus water out Sider's notion of structure. This carries over to the s-norm, which would then only require that we use *some* terms to talk about the structure, not the structural terms themselves. This would go directly against Sider's claim about there being a "privileged description". Instead, Sider's idea seems to be that some *primitive* terms are better than others to describe the world. "Green" and "grue" are different primitive terms, and it is better to use "green" (and "blue") than "grue" (and "bleen") to describe what there is, even though the expressions are interdefinable. This is because " $\text{struc}(\text{green})$ " is true and " $\text{struc}(\text{grue})$ " is false. Thus " $\phi$ " must be a primitive term of the language it is part of.

That Sider understands "term" in the s-norm as primitive terms in the language of those engaged in inquiry is also supported by his discussion of logic (2011, chapter 10). Sider raises the question of whether there is logical structure. As we have seen, we can ask whether any grammatical category is structural, so which of the logical constants carve at the joints? For instance, is it "or" or "and" that carve at the joints? As we know, if we also have negation, then these expressions are interdefinable, so it would seem that if both expressions carve at the joints, there is a certain redundancy to the world's structure (2011, p. 218f). Sider is not fond of such redundancy, but eventually allows it on the ground that there is no principled reason for favoring any of the logical constants. If Sider thought  $\text{struc}(\phi)$  took complex values, this discussion wouldn't make much sense, because the interdefinability of the notions would imply

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<sup>134</sup> The following description defines "green" in the grue/bleen-language: Something is green if and only if it is either grue and has been examined before  $t$ , or bleen and has not been examined before  $t$ .

that both “or” and “and” could be taken to talk about the same structure. This suggests that Sider believes that “ $\text{struc}(\phi)$ ” can be true only if “ $\phi$ ” is a primitive notion.

Sider connects the terms in the “book of the world” to the s-norm: it is better to use structural terms. Based on what I’ve argued, the discussion of the s-norm must be carried out under the assumption that the s-norm has the following content: To fully achieve the goal of inquiry, it is better to think and speak in joint-carving terms, where “terms” are primitives.

On this view, joint-carving terms are better because there is some kind of “match” between the terms used to describe structure and structure itself. It might be objected that it is hard to get a grip of this notion of “matching” when we potentially have entity-free structure, but able philosophers have raised epistemological objections elsewhere,<sup>135</sup> and I wish to keep the focus on the justification of the s-norm required to uphold normative quantifier invariantism, and so will not press such objections (see Sider (2011, chapter 2)).

We have seen that Sider’s defense of ontology against quantifier variantism requires two things: (1) that there is in fact quantificational structure (the structure claim), and (2) that there is a best way to describe this structure (the privileged description claim). Before moving on to criticism of the s-norm, I will remark that one cost of adopting Sider’s response to quantifier variantism is that it brings with it heavy theoretical commitments. One must endorse a specific metametaphysical outlook that requires substantial assumptions about the correct method for metaphysics, what “correct representation” comes to (see section 4.4.1 below), about which questions are substantive (e.g. that it is a substantial question which of the logical constants carves at the joints), and so on. In contrast to this, the arguments of chapter 2 and 3 didn’t require such a substantial “reconceptualization” of metaphysics. Compared to the arguments in chapter 2 and 3, then, Sider’s reply to quantifier variantism requires a considerable theoretical buy-in. Sider’s reply makes significant assumptions that one must make up one’s mind about. There are strengths and weaknesses associated with such a theory-dependent reply. Sider’s theory is massively powerful and has a wide range of applications, but on the other hand, if you want to defend ontology on a budget, his offer is not for you.

Another thing that should be kept in mind when making up one’s mind about whether to adopt the framework is that it has been developed quite recently and is currently undergoing scrutiny.<sup>136</sup> Both the assumptions and the consequences of Sider’s theory might turn out to be problematic after closer inspection. The only new commitment one must make by adopting Sider’s theory that I will critically discuss is the new epistemic norm relevant to normative

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<sup>135</sup> See the symposia in *Analysis* 73 (2013, pp. 716–750) and *Philosophical and Phenomenological Research* 87 (2013, pp. 708–732). See also Chalmers (2009).

<sup>136</sup> See *inter alia* the symposia referred to in footnote 135.

quantifier invariantism. I don't think that the failure of this norm is sufficient to undermine the whole program (see section 4.5), but I hope the following discussion is a small contribution to the current project of exploring Sider's rich work.

#### **4.4 Arguments for the s-norm**

If Sider's defense of ontology against quantifier variantism is a form of normative quantifier invariantism, then for it to be successful it must justify that it is better to think and speak with a joint-carving quantifier. This claim crucially depends on the existence of an s-norm that carries over to quantification. Sider believes that this norm is a basic constitutive norm for belief, and not derivable from other values (2011, pp. 61–62). Since the postulation of this norm is an epistemic novelty that diverges from orthodoxy when it comes to the aim of inquiry and belief, so we should ask for relatively good arguments for believing in it. To put my cards on the table, I'm skeptical about such a norm, and think Sider's arguments for it are unsuccessful. If Sider's defense of ontology through Ontologese depends on this norm, then I'd want look for other options. Given the centrality of the norm to Sider's (2011) project, it is puzzling that he spends relatively few pages discussing and defending it.<sup>137</sup> On the positive side, this makes it possible to evaluate his four arguments for the s-norm within the bounds of this thesis.

##### **4.4.1 Knee-jerk Realism**

The first argument for the s-norm is somewhat implicit, derivable from what Sider says about *knee-jerk realism*, which is an “unargued presupposition of this book” (2011, p. 18). Sider (2011, p. 18) describes knee jerk realism as the conviction that the point of human inquiry is to “*conform* itself to the world”; the world is “out there”, and our job as inquirers is to wrap our minds around it. Sider concedes that this is a “vague picture rather than a precise thesis” (2011, p. 18). Basing an argument on an unargued presupposition may be considered problematic, but as we will see, the crucial premise of the argument is plausible and likely to be embraced by many philosophers of a realist disposition, so the argument is worth discussing.

The argument is this (Sider 2011, p. 19). Let A be the set of true sentences in the language of complete physics. Consider these two sets of propositions: P is the set of propositions expressed by A under their intended interpretation. The second set, S, consists of “scrambled” propositions construed by reinterpreting all the non-logical symbols of the language of physics under an arbitrary permutation of the totality of object (Sider 2011, pp. 23–35). This is analogous to the arbitrary permutations driving Putnam's paradox (1980). Thus S contains proposi-

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<sup>137</sup> It is of course a little hard to say exactly how many pages are devoted to arguing for the norm, and perhaps Sider will say that somehow the whole book is a defense of the norm because he considers it central to his project (Sider 2011, p. 65), but the pages devoted to arguments for the norm are pp. 19–20 and pp. 61–65, a totality of 7 pages.

tions that are true,<sup>138</sup> but do not use the non-logical symbols of physics and do not conform to how the world is “out there”, because S is a description of reality using arbitrary terms. In Sider’s words, “P constitutes a better description of reality than S”; to deny this would be to admit that there is nothing “mandatory about physics” (2011, p. 19), and this is in conflict with knee-jerk realism. This betterness is supposedly objective, and Sider concludes that the reason for this objective betterness is that the propositions in P are cast in joint-carving terms, while the propositions of S are not (Sider 2011, p. 19). Thus the s-norm *explains* why we prefer P over S. This is the argument from knee-jerk realism.

Sider formulates this as an argument having knee-jerk realism as a premise, and it is sound if we take Sider’s claims about the implications of knee-jerk realism to be true. But as Sider admits, knee-jerk realism is a vague picture, and not the best premise to build one’s arguments on. Instead of arguing against knee-jerk realism, I will argue that we can accept the important insight of this argument without thereby accepting that one must *use* the terms of physics when talking about these aspects of reality. The important insight is that physics is privileged (“mandatory”) when it comes to *discerning* what certain aspects of the world are like, but that the language of physics is not mandatory when *describing* those aspects. If the insight can be preserved without accepting the s-norm, there seems to be nothing in the argument that supports the s-norm.

It is important for this criticism that physics tells us which predicates to use when describing the world, and which properties are instantiated.<sup>139</sup> I will therefore consider whether the claim that physics says that some properties are structural entails that it is *better* to use certain predicates to describe what there is, which is what the s-norm comes to.

To do this, I will consider an argument from another work of Hirsch. Hirsch (1997, chapters 3 and 4) investigates whether it follows from the assumption of Lewisian naturalness that we should use some concepts rather than others, i.e. that there being perfectly natural properties entails that some words are better than others. Hirsch uses illustrative examples to bring out the problems with such inferences. None of these examples are in terms of predicates from physics, but I will get to that in a second. Here is a recent statement of such an example:

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<sup>138</sup> Because reinterpretation under a permutation preserves truth (Sider 2011, p. 19).

<sup>139</sup> A complete physical description of the world will not only consist of predicates. For instance, it is a significant question whether it includes temporal operators, and it very plausibly will contain quantifiers. However, it doesn’t seem that we can infer anything about the quantifier employed without auxiliary premises (see Sider 2011, pp. 186–187 for one such argument). The fact that physics uses a quantifier only suggests that any complete description of reality must use a quantifier that quantifies over the fundamental physical particles, fields, or what have you. It would seem that different ontological languages agree on the fundamental physical description of reality, as they usually agree on the distribution of fundamental particles. For potential complications, see section 4.5.

Imagine people who are cognitively disposed (“hard-wired”) to apply words to disjunctions of natural properties and to *realize* that this is what they are doing. Their language is made up of such words as “cubound” and “rindical” that refer, respectively, to things that are either cubical or round and things that are either round or cylindrical. One completely misses the point of my question if one fails to understand that these people *agree* with us about which properties are natural and which unnatural. It’s just that they have words for the unnatural ones and complex terms for the natural ones. (Hirsch 2013, pp. 711–712)

Suppose that the words “cubical”, “round” and “cylindrical” stand for natural properties. These foreigners can talk about the same natural properties as we do. For instance, “cubical” can be expressed by “cubound and not rindical” and “round” by “cubound and rindical” (Hirsch 2013, p. 712). In a word, the descriptions are truth-conditionally equivalent. Even though their way of expressing these natural properties is unfamiliar, counterproductive (for us), and may initially strike us as irrational, it is unclear why these imaginary foreigners are making a rational *mistake*, as Sider believes happens in such cases (Sider 2011, p. 2). Is there an s-norm here that our foreign friends are breaking? They are hard-wired to think a specific way, and thus formulate their theories in a way that suits their cognitive disposition, but nonetheless accept that their concepts thereby don’t carve at the joints. But they are still able to express and grasp the joints. It seems wrong to hold their cognitive disposition against them, judging them irrational merely because of the way they prefer to formulate their theories.

Does changing the example to physical predicates change this verdict? Suppose that these people didn’t have physicists themselves, but instead were told about our physicists, their sophisticated methods and epistemic virtues. Being rational, they would presumably agree that the properties discerned by physicists are more objective than the gerrymandered ones they usually talk about. This is analogous to how we understand that photon wavelength is more fundamental than colors, but as long as they can define predicates for the perfectly natural properties without using the exact predicates of physics, yet still acknowledge the priority, or *mandatoriness*, of physics, there seems to be nothing epistemically amiss here.<sup>140</sup> I submit that Sider’s scrambled propositions are not epistemically inferior, and that we can acknowledge the privilege of physics in discerning reality without accepting an s-norm.

Furthermore, it seems that within science itself, it is truth-conditional equivalence that matters. When individuating theories of physics, we do so in terms of truth-conditional equivalence. There are several provably truth-conditionally equivalent formulations of classical

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<sup>140</sup> Another matter is whether this is practically plausible. Could these foreign people really come to have predicates that are adequate to describe the world’s physical structure without using the predicates we tell them about from our physics? We may instead consider some other foreign people that are cognitively disposed to immediately, and as a result of hard-wiring, create disjunctive properties out of every physical predicates we teach them: when we teach them “mass” and the sentence “if two objects are of different mass they will attract other objects with different gravitational force”, they will create the predicate “schmass” which means “mass or everything is self-identical”, but their schmass-theory will be of the same predictive value as our theory.

mechanics, for instance. The Hamiltonian formulation is perhaps more elegant than Newton's, but as long as physicists don't distinguish these in matters of descriptive power or level of precision, presumably we wouldn't want to distinguish one of these as *better* because of some novel s-norm. Relatedly, Newtonian mechanics and Einstein's general relativity have the same predictions for relatively large objects at relatively small speeds, so Newtonian physics is used for many practical purposes because it is simply easier to work with and compute than general relativity. Within a certain domain, they are equivalent, but the fact that we choose to use Newtonian physics for practical purposes doesn't mean it's any better objectively speaking.

Here is a possible objection: Sider writes that a goal of his book is to argue that "ideology matters" (2011, p. vii). A theory's ideology is as much a part of the content of the theory as its ontology, he claims, because the theory "represents the world as having structure corresponding to its primitive expressions" (Sider 2011, p. viii). While this claim would warrant claiming that the predicates of physics are privileged and better for describing the world, this claim about ideology itself seems to be begging the question against the foreigners: if they are hard-wired to think of the world in a way that doesn't correspond to the joints, but nonetheless realize this and formulate their theories using their non-structural concepts to talk about the structure, it is unreasonable to say that their chosen ideology *represents* the world *as* having the structure corresponding to their primitive expressions. Rather it would seem that our foreign friends should insist that it isn't a consequence of choosing ideology that those primitives represents the world as having structure corresponding to the primitive expressions of the theory.

The foreign people would presumably hold that they only make statements about structure when they say  $\text{struc}(\varphi)$ , where " $\varphi$ " is some complex expression in terms of their non-structural primitives, not when they state their primitives. Their "book of the world" would thus be in terms of primitives that *don't* carve at the joints themselves. What Sider requires is that all primitives in Ontologese are themselves structural. The notion that the primitive ideology of a theory must itself carve at the joints, instead of the whole theory expressing the joints, seems to presuppose an s-norm, and thus does not provide independent argument for an s-norm. If we reject the s-norm, we should deny that we implicitly take a stance on the world's structure when we choose to formulate a theory in some ideology.

It would seem, then, that we could grant that physics is mandatory when exploring the world in the sense that we must *talk about* the structure physics discovers, but this does not entail that there is an s-norm, thus we don't have to carry out that talking in any particular ideology. For Hirsch's example seems to show that we can *talk about structure without using structural concepts*. This distinction between structure on the one hand, and there being a



rational requirement to talk about it in a given way (the s-norm), will be important in the following. I will time and again return to it to deflect Sider's arguments.

#### 4.4.2 Beliefs aim to conform to the world

The second argument is contained in this paragraph:

First, the aim of joint-carving can be seen as having the same source as the aim of truth: beliefs aim to *conform to the world*. Here is a simplified but intuitive picture. The realist about structure thinks of the world as coming “ready-made” with distinguished carvings. By analogy with the notion of a mathematical structure, think of The World as a structure: a set  $E$  of entities together with a set  $R$  of relations over  $E$  (think of the relations here simply as ‘tuples of members of  $E$ ’). Now, ignoring partial belief, it is natural to think of the beliefs of a subject,  $S$ , as consisting of the representation of a structure: the subject represents there being objects,  $E_S$ , together with a set  $R_S$  of relations over  $E$ . Given this picture, it is utterly natural to think of full conformity to The World as requiring  $(E_S, R_S)$  to be identical to  $(E, R)$ . Conformity requires the believer to represent the structured world exactly as it in fact is, and thus requires the represented relations  $R_S$  to be identical to the world's structuring relations  $R$ . Thus if belief aims to conform to the world, and if belief and the world are both structured, belief aims not just at truth, but also at the right structure – truth in joint-carving terms. (Sider 2011, p. 62)

This argument presupposes that the source of both the s-norm and the truth-norm is “conformity to the world”. The conformity-claim is supposedly part of knee-jerk realism, but I intend to evaluate it on independent grounds due to the vagueness of knee-jerk realism. From the claim of “conformity”, Sider argues that because structure is part of the world, our beliefs should “conform” to the world's structure, so we should think in terms of structural notions.

It is not obvious that the *source* of the aim of truth is this more fundamental conformity norm (more on this below). It is a quite common assumption in the literature on the aim of belief that there is a *sui generis* truth-norm for belief,<sup>141</sup> but Sider's argument seems to reject this. The derivation of the s-norm from the conformity-norm also seems to go against Sider's own view that the s-norm is a basic norm not derivable from others.

Setting these problems aside, why should we accept the conformity-norm in the first place? Sider's argument is that the conformity norm is the source of the s-norm, so we must already accept the conformity norm to be persuaded by this argument. One notable problem is that Sider hasn't provided any independent arguments for the existence of such a norm, only claimed that it is a part of the unargued presupposition of his book, namely knee-jerk realism. I'll nonetheless evaluate the plausibility of such a norm.

If there is a conformity-norm, then it is the source both of the s-norm and of the aim of truth for belief (the truth-norm). So we can argue against the plausibility of the conformity-norm by arguing that it is implausible that it is the source of the truth-norm.

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<sup>141</sup> See, for instance, most of the essays in Chan (2013).

The main problem is that it is unclear what “conformity” comes to in the context of truth, and thus that it is unclear that the conformity-norm is the source of the truth-norm. First, it seems that we understand truth better than we understand conformity, for the notion of “conformity to the world” seems to be just another analysis of truth, alongside deflationist accounts, coherentist accounts, and correspondence accounts, and so on (conformity is probably a beefed-up version of correspondence theories). Thus we are more committed to the validity of the truth-norm than we are to any analyses of truth that may entail the truth-norm.

Furthermore, it is implausible that truth can always be analyzed as conformity, which is a requirement of the truth-norm having the conformity-norm as its source. For instance, in what sense does the true sentence “There are no unicorns” conform to the world? There is nothing in the world for it to conform to, for there are no unicorns, but the sentence is nonetheless true. Correspondence-accounts of truth have a general problem with negative existential assertions, and it would seem to carry over to the conformity analysis of truth. It is also unclear how the sentence “There are two ways to win this game” conforms to the world. What is the “way”-structure that the sentence latches onto, and if there is no such structure, what makes the claim “conform”? We nonetheless think these sentences are true, but they don’t seem to conform to the world. The goal here is not to refute conformity accounts of truth, but to illustrate the sense in which they are more problematic than truth itself. We understand truth better than conformity, which seems to undermine that the conformity-norm can be the *source* of the aim of truth.

Even if such questions can be answered, it seems tendentious to say that the *origin* of the truth-norm for beliefs is conformity all the time this (1) seems to presuppose a specific account of the nature of truth, and (2) we understand truth better than conformity.

Sider may object that I fail to understand his claim about conformity. Perhaps there is a sense in which we are making a mistake if we don’t use the right concepts when we describe the world. But such claims make me recall the hard-wired foreigners, who are as conscientious inquirers as we are, but are hard-wired to think and talk in non-structural ways. I don’t see how pointing to this mysterious “conformity-norm” would make them realize that they are making a rational mistake. Perhaps I’m not the right kind of knee-jerk realist, but then I’d rather reject Sider’s knee-jerk realism and accept some other form of realism, for I’m decidedly concerned with having true beliefs, but I’m not so sure whether I think they must “conform”.

When it comes to Sider’s “natural” belief that our beliefs must have the form ( $E_S, R_S$ ) to match up with reality’s structure ( $E, R$ ), this is dependent on two claims: (a) beliefs aim to conform to the world, and (b) beliefs are in general structured in this way (cf. the last sentence of the quoted passage). (a) would seem to depend on there being an intelligible conformity-

norm applicable to belief, which I have attempted to object to. And (b) is a substantive claim about the “form” or “structure” of our beliefs. There are several accounts of belief, and it is a controversial and difficult topic how much structure they have, but it would seem that Sider is not deriving the structure of belief from an analysis of belief itself, but rather derives it from the s-norm. On another note, as I have stressed, we can have true beliefs about the world’s structure without the primitive constituents of language or thought “matching” the world’s structure, so it seems unimportant that our beliefs actually conform to the world. The example seems to be working more as an illustration than an argument. If our beliefs have this structure, it would seem to be a consequence of the alleged constitutive aim of belief to conform to the world, and not something we arrive at by reflection on belief. But it seems simpler and more in accordance with our common assumptions to reject (b), insisting that without further justification of the s-norm our beliefs doesn’t have this form.

I think we based on this should conclude that there are too many unanswered questions and problems with the conformity norm to at this point take it as sufficient grounds for believing in the s-norm. That’s not to say that there is no argument for an s-norm in terms of a basic conformity-norm, but I don’t see how such an argument would go.

#### **4.4.3 Epistemic value**

The third argument Sider puts forward is a set of observations that are supposed to persuade us that claims in joint-carving terms are better than claims that are not. To criticize the argument, it will be convenient to have the whole passage in front of us:

... we think of scientific discovery as satisfying the aims of inquiry particularly well; why? Answer: it is because scientific discoveries are phrased in particularly joint-carving terms. Relatedly, we think of truths that are stated in extremely non-joint-carving terms – for example, the scrambled propositions of section 2.6 – as being comparatively worthless. Relatedly, imagine (or recall) first coming to believe that morality, beauty, justice, knowledge, or existence is a mere projection of our conceptual scheme – that the truth in these domains is conventional, subjective, or otherwise nonsubstantive. Why does that feel so deflating; why does it diminish the urgency of finding the truth; and why does it diminish the value of the truth once found? Answer: though we might not put it exactly thus, our original picture in these lofty domains is that of joint-carving. Morality, beauty, and the rest are built into the nature of things, we naively think, rather than being mere projections. Giving up on objectivity means giving up on joint-carving, and hence diminishes the value of truth. (Sider 2011, p. 62)

The passage appeals to how we “think of scientific discovery”, our experience of some truths being “worthless,” and a feeling of deflation when we come to believe that certain truths are projections of our conceptual schemes to show that we care about the truth in joint-carving terms.

There are three short arguments here of essentially the same form: an s-norm is the *best explanation* of these different beliefs. For instance: There being an s-norm is the best explanation of why we think scientific discovery satisfies the aims of inquiry particularly well. Thus my strategy in arguing against Sider will be to attempt to show that there are other, equally good explanations of our beliefs. This is not a watertight argumentative strategy, but seems to be of the same kind as Sider's own arguments. After considering the three cases, I question whether our beliefs, feelings or otherwise subjective dispositions provide good grounds for drawing general conclusions about the s-norm.

First, it is not clear that our impression of the epistemic success of science is related to scientific discoveries being cast in joint-carving terms. This is a very general claim, and it is thus hard to know whether my alternative explanation accounts for what Sider has in mind, but I will try. Here is an alternative explanation of this data point: Science is the best we have when it comes to inquiry, and we care about the truths it discerns because we know they are epistemically well grounded. Science absorbs the best epistemic methods and institutionalizes and refines them. For instance, science is based on trial and error; experiments hold everything except what one is testing constant; science develops systematic and explanatory powerful theories, and revises them in light of new evidence.<sup>142</sup> To me, it would seem that these merits of science constitutes an equally good explanation of why we think of "scientific discovery as satisfying the aims of inquiry particularly well" (Sider 2011, p. 62). Sider infers the s-norm from this data point, but it would seem my explanation could account for it as well. As my suggested account brings in fewer commitments than Sider's (which requires the s-norm), one should prefer mine. Objection: There is room for more than one explanation, and an s-norm also has a role to play in explaining our attitude toward science. Answer: Sider's formulation doesn't suggest that he thinks there are several explanations, but if we grant that, we can answer that also the foreign people I constantly return to would relish science, and that they can relish science without conceding that they do so because of the *terms* science uses to express their discoveries. They value science because of the value of the discoveries of science. It is the content, not the form, of such discoveries that matters.

The second point, that truths stated in non-joint carving terms are "worthless", seems like a red herring. Our foreign friends that uses "cubound and not rindical" to say "cubical" and thereby talk about the (supposedly) natural properties, and regard the propositions about structure as valuable, but that is because they express something objective about reality, and has nothing to do with the *terms* they use. An alternative explanation of our belief that scram-

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<sup>142</sup> This is not to say that there aren't significant questions in the philosophy of science about how science proceeds, what scientific discoveries really are, and so on. But I hope what I've said is rather uncontroversial.

bled propositions are worthless is that they have no practical value to us. They are framed in primitive vocabulary that's unfamiliar to the brink of unintelligible to us, so it's not surprising that we don't care about them. But that doesn't mean that our belief is grounded in us accepting an s-norm. Rather, our attitude towards "scrambled" propositions could be grounded in our cognitive dispositions and such propositions' lack of instrumental value to us. Besides this, I refer back to what I said in response to knee-jerk realism in section 4.4.1 above.

The third part of the argument – that we lose interest in aesthetics, ethics, and so on when we realize that our aesthetic or ethical beliefs are merely projections of our conceptual schemes – is also unsuccessful. Perhaps this is a correct empirical observation about philosophers (and laymen in general), but if we want to frame this in Sider's vocabulary, it seems that we lose interest when we understand that there *are no joints*, not because we realize that there is no privileged description of these joints. Sider seems to conflate these issues. Furthermore, what we care about is that our beliefs are not merely *projections*, but structure is not necessary to explain this. Structure is but one way of cashing out what it means to be objective, and what we care about is that beliefs concern something objective. As the discussion of moral facts in metaethics shows, we certainly don't have to accept Sider's framework of structure to accept that there are moral facts (potential problems with this notwithstanding). Personally, at least, I would care as much about ethics in a scenario where there is an s-norm as in one where there isn't one. What's important to us is that there is something objective about these disciplines, not that there is only one "correct description" of the ethical or aesthetic facts.

Is this reply uncharitable to Sider? I claim that Sider is making the mistake of not distinguishing between the structure-claim and the privileged description-claim, and that he is taking our intuitions about objectiveness in general in support of the s-norm. Implicitly, I'm claiming that he doesn't really see these distinctions in these cases. This might be because we have opposing views on the independence between the structure-claim and the privileged description-claim, but the arguments I'm discussing are from the section "Epistemic Value" of Sider's book, a section devoted to arguing for the s-norm being a constitutive aim of forming beliefs (2011, p. 61). I will therefore take it that Sider thinks these observations support an s-norm over and above the claim of structure and the epistemic goal to describe the structure completely, which is just an instance of the truth-norm.

Having suggested some alternative explanations of the attitudes, we should ask whether Sider's argumentative strategy is a good one for determining whether there is an s-norm. Are the kinds of beliefs and attitudes Sider appeal to feasible data to establish whether there is an s-norm in the first place? The s-norm must be a general epistemic norm, applicable to all rational agents, independently of their cognitive or biological make-up and hard-wiring. This

is necessary because the norm is supposed to be a constitutive aim of belief that reflects something about what it means to be an inquirer and represent reality “as it is”. Thus the arguments for it cannot trade on humans’ cognitive dispositions or instrumental concerns.

In light of examples like the foreign people of Hirsch, we should be suspicious of the value of our attitudes providing evidence for an s-norm. That we prefer some descriptions over others seem to be more a matter of our own cognitive dispositions than of a norm applicable to all rational agents. Some of Sider’s remarks seem to suggest that we wouldn’t be able to make rational predictions and giving correct explanations if we don’t use the “right concepts”.<sup>143</sup> But this claim would seem to trade on the contingent cognitive dispositions of us humans, and does not imply that it is impossible to make rational predictions without joint-carving terms. The s-norm is supposed to be completely independent of our cognitive dispositions or preferences, but it is implausible that our attitudes and beliefs are independent in this way. Thus we should be somewhat skeptical of the weight of Sider’s arguments.

The *consequences* of there not being such a norm is perhaps not as devastating as Sider seems to think (Sider 2011, p. 65). For our foreign friends do accept structure, and they can talk about it and care about discovering and describing it. A final point can be made about these examples. If the attitudes Sider appeals to in support of our implicit acceptance of an s-norm should be taken as evidence for such a norm, we must note that they are all about there being some *predicates* that are better to use, not about quantifiers. I’ve been using predicates in my examples because we seem to have a firmer grasp of them than we do on “quantificational structure” and because Sider also focuses quite exclusively on predicates when arguing for the s-norm, but we may nonetheless note that most of the evidence Sider has put forward for an s-norm so far trades on predicate structure. So it is unclear that even if the arguments just surveyed establish an s-norm, that norm can be said to carry over to ontology or other parts of metaphysics.

#### **4.4.4 A series of scenarios**

The last argument Sider gives for the s-norm takes as its starting point five scenarios where the match between our beliefs and reality’s structure is gradually eroded (2011, p. 63). The idea behind the argument is to reflect on our opinions on what we care about when forming beliefs. The consequence is supposed to be that we don’t just care about our beliefs being merely true – we also care about “truth in joint-carving terms” (Sider 2011, p. 63).

I will not present all the scenarios here and repeat Sider’s remarks about them, because the argument seems to suffer from the same shortcomings as the arguments discussed above;

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<sup>143</sup> Compare Hirsch (2013, p. 715).

there are other alternative explanations just as salient as there being an s-norm. By maintaining a strict distinction between the structure-claim and the privileged description-claim we see that it's hard to find evidence for the s-norm.

Consider the contrast between Scenario 1, in which “the physical world is pretty much the way we think it is; it includes physical objects in addition to spacetime” and Scenario 2, in which “the physical world consists of nothing more than propertied points and regions of spacetime” (2011, p. 63). The suggestion is that if we suppose we have the beliefs we now have in these scenarios, we fail more epistemically in Scenario 2 than 1, and Sider thinks this shows that we care about the truth in joint-carving terms. But this might be explained equally well by the fact that our beliefs about the structure of reality are more wrong in Scenario 2 than in Scenario 1. What we care about is that we have true beliefs about what the structure *is*. In scenario 2, our ordinary beliefs about the world are to some extent false. Again we may appeal to our foreign friends who care equally much about discerning reality's structure, but employ non-joint carving terms. They would also think that they are worse off epistemically in Scenario 2 than Scenario 1, but we wouldn't say that this is because they use non-joint-carving terms when expressing their beliefs. Rather, we are worse off in scenario 2 because we don't have correct beliefs. There is nothing in the scenarios that suggests that our “intellectual failure” occurs *because* we don't think in joint-carving terms.

Consider also the contrast between Scenario 4, where “our ordinary beliefs are caused by The Matrix, a computer simulation that directly stimulates our brains while our bodies lie in stasis” and Scenario 5, where “I am a disembodied brain floating in an utterly empty space; the changes in my brain that give rise to my “mental states” happen purely by chance” (Sider 2011, p. 63). Even though we have false beliefs in both these cases, we are epistemically worse off in Scenario 5 than in Scenario 4. But this is not best explained by us caring about truth in joint-carving terms. The belief-forming process in Scenario 5 is random, and it moreover generates “beliefs” that hardly can be said to have content at all. In Scenario 4, on the other hand, we can clearly imagine different ways for the belief-forming process to function at least non-randomly and have content that may be about *something* (perhaps aspects of the computer program). These examples can be clearly distinguished from Scenario 1, where our beliefs-forming processes (most centrally perception) is functioning as we believe it does, which puts us in a good epistemic position. This appeal to the relative success of our belief-forming processes seems apt to explain the epistemic differences between the scenarios. In general, then, Sider's argument from scenarios seems unsuccessful in establishing the s-norm.

After having considered these four direct arguments for why there is an epistemic value in thinking in joint-carving terms, I conclude that they don't provide good enough reasons for

believing there to be an s-norm in addition to the good old truth-norm. In the absence of good positive arguments, our fallback position should be to reject this novel epistemic norm.

Most of Sider's arguments are based on examples where we are invited to see that we already implicitly care about the truth in joint-carving terms. Even though I do feel the pressure to prefer joint-carving descriptions, I have not been able to see that there are sufficiently good reasons for believing these instincts as evidence of a general s-norm. My main argument against such a norm has been that it is wrong to attribute irrationality and "epistemic shortcoming" to agents hard-wired to think in non-joint-carving terms, but who nonetheless care about structure. Furthermore, it seems that we can come up with other equally good and plausible explanations of our intuitions about epistemic success in the cases Sider asks us to consider, and we should also be somewhat skeptical of whether our intuitions are good guides to objective epistemic norms beyond truth, because it might simply be the unfamiliarity of the terms that creates our intuitive reactions. Finally, we may restate that the resulting view isn't as bad as Sider seems to think. We can accept that Sider has given us good reasons to think that we care about whether we have true beliefs about the world's structure (if there is any such), but this is simply a fall-out of the truth-norm, and does not provide evidence for an s-norm.

#### **4.5 Saving ontology without the s-norm?**

Above I argued that Sider hasn't given us good enough reason to believe that there is an s-norm. If there is no s-norm, Ontologese cannot be a metaphysically privileged language *in virtue of* there being an s-norm, which seems to be Sider's main reason for believing that Ontologese is privileged. This is an important result vis-à-vis Hirsch's arguments, because it implies that Sider's flavor of normative quantifier variantism is unsuccessful, or at least that we need better arguments to accept the privilege of Ontologese for purposes of ontology. By analyzing Sider's arguments for the s-norm, we have gotten better insight into some of the problems of cashing out and justifying one sense in which "truth is not enough".

But even if there is no s-norm and normative quantifier invariantism is false, perhaps Ontologese can be "privileged" in another way. After all, one can use Ontologese to talk about quantificational structure if there is any. As I've flagged, I haven't discussed whether there is any quantificational structure, but if there is, then by definition any complete description of reality must use a quantifier that has expressive resources to talk about all the quantificational structure. If Ontologese can be used to talk about the quantificational structure, then Ontologese is privileged after all, but in a less exotic way than the s-norm promised. The privilege of Ontologese would then merely be a fall-out of the truth-norm and the goal of providing



a complete description of reality, for reality contains quantificational structure. This section explores this alternative version of Sider's response to Hirsch.

Sider (2011, pp. 180–189) argues that there is quantificational structure: any complete description of reality must use quantifiers. This means that we can stipulate that there is a joint-carving quantifier (Sider 2011, pp. 171–172). Given that there is quantificational structure, structure is “something in the world”, and thus languages that don't use the structural concepts, for instance presumably English, seem to *miss out*, especially if there is more quantificational structure than English talks about. In fact, Hirsch makes some comments on a position that isn't too unlike this. He writes that quantifier variantism isn't compatible with the view that there is a “*unique* sentence-shaped thing in the world” (Hirsch 2011, p. 79).<sup>144</sup> It would seem that Sider's focus on some *terms* being structural, thus expressing something objective about reality, has something in common with such a view. On Sider's view, there is an objectively best way to “write the book of the world”.<sup>145</sup> Quantificational structure thus threatens *weak quantifier variantism* (instead of strong quantifier variantism) because it appears that there are some truths we can only express using Ontologese.

Whether this is part of what Sider has in mind when he says that Ontologese is better than other ontological languages I do not know, but at least this reply doesn't depend on the s-norm.<sup>146</sup> Much of what he says about “truth not being enough” and the arguments for the s-norm suggest that this isn't Sider's preferred position, but it might still be enough to defend ontology against Hirsch. It is interesting that there seems to be a difference between quantificational and predicate structure here. For while in the case of predicate structure we can use disjunctive or other non-joint-carving predicates to talk truthfully and exhaustively about the world's distinguished predicate structure, it is not obvious how we would do that for quantificational structure. On the assumption of quantificational structure, how does things stand with the claim that all ontological languages are truth-conditionally equivalent?

Hirsch's (2013) suggests a response on behalf of the quantifier variantist that skirts this problem elegantly: he suggests that all quantifiers are structural. In that case, we don't have this problem, though one may object to the massive redundancy of such structure (Sider

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<sup>144</sup> This is originally a formulation from Putnam (1993, p. 301).

<sup>145</sup> These formulations ignore what was clearly stated above: that structure isn't a *thing* in the world.

<sup>146</sup> Thus I don't really know whether Sider *should* be classified as a normative quantifier invariantist or simply a quantifier invariantist. In the grand scheme of things, this isn't very important, but when I chose to write this thesis I had to make some dispositional choices. One of these was how to present and classify Hirsch's views and arguments, and I found that the distinction between weak and strong quantifier variantism, and consequently between quantifier invariantism and normative quantifier invariantism, was both tidy and illuminating. When describing Sider's view, these categories are perhaps less apt, but I hope that I've been able to give a comprehensible account of Sider's view nonetheless. In any case, it is interesting to consider different ways of understanding Sider's position.

2013).<sup>147</sup> However, if we want to assess Sider's defense of ontology, and also to understand the relationship between quantifier variantism and Sider's structure, we must evaluate whether Ontologese is in some way better than alternative quantificational languages in the absence of an s-norm.

Surprisingly, an asymmetry between "big" and "small" languages not unlike the asymmetry I argued against in section 2.3 seems to reemerge here. Suppose that Ontologese uses a quantifier we call  $Q$ , and only quantifies over fundamental particles. It would then seem that someone speaking a "bigger language", for instance Universalese, could say " $\text{struc}(Q)$ ", but don't feel pressured to actually *speak* Ontologese, because (i) there is no s-norm, and (ii) she can talk about all the quantificational structure (the fundamental particles) in her own language. Universalese gives the impression that there is *more* quantificational structure than there really is, so is in a way imprecise, but as long as she agrees that  $\text{struc}(Q)$ , the absence of a s-norm doesn't push her towards revising her language, for there is nothing wrong about not "matching" reality's structure with one's language. As I noted in relation to ideology in section 4.4.1 above, choosing to use a certain ideology doesn't seem to entail that we thereby *represent* the world as having the structure of that ideology. So Universalese has the same metaphysical merit as Ontologese.

On the other hand, if the quantifier  $Q^*$  in Ontologese quantifies over any arbitrary mereological sum, then someone speaking Nihilish has a problem. For if a speaker of Nihilish utters " $\text{struc}(Q^*)$ ", it would seem that she concedes that her language is missing out on describing reality completely, for  $\text{struc}(Q^*)$  implies that there are mereological sums in the objectively best meaning of the quantifier, so there are mereological sums "out there", but Nihilish does not quantify over them as such. So Nihilish is expressively impoverished.

This is somewhat puzzling, but upon reflection it should perhaps be expected. If one adopts such a substantial notion of reality as Sider's theory of structure seems to imply, Hirsch's arguments from truth-conditional equivalence of chapter 1 starts to erode. For with the assumption of quantificational structure, one is close to insisting that there are these things out there that one must quantify over to completely describe reality – there are "sentence-shaped facts" that must be accommodated in a complete description of reality. Quantificational structure seems to supply the "objectivity" of certain objects that any complete description of reality must countenance, independently of which language one is speaking, en-

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<sup>147</sup> Another potential problem is that it would seem to be mandatory to use *every* ontological language if one aims to describe the world completely, because there is so much structure one needs to describe. This seems somewhat puzzling: that there is so much structure in the world that we can never accomplish to describe it all (because we can never use all languages). In Sider's (2013) reply to Hirsch, it is pointed out that there would seem to be a lot of redundancy, and that there is a general epistemological presumption that non-redundancy is more plausible than massive redundancy, which he relates to the general epistemic norm of believing the comparatively simplest theory.

tailing the falsity of quantifier variantism because we must inquire into the quantificational structure to find out whether different ontological languages really are truth-conditionally equivalent.

We might consider how this differs from Sider's official view. If there is no s-norm, then it is not necessary to use the privileged quantifier to describe what there is. It would seem to be sufficient to have a language that quantifies over more things than the joint-carving quantifier does, as the example with Universalese above suggested.<sup>148</sup> On the other hand, to *determine* what there is (in the joint-carving sense of "there is"), one must use a joint-carving quantifier, for the quantifier of Universalese is *defined* to quantify over arbitrary mereological sums, and thus doesn't seem to leave any room for ontological inquiry and discovery. This sits well with the discussion in 4.4.1 above: to discern what there is, we must do physics, but we don't have to use the primitives of the theories of physics when describing what there is. By using a joint-carving quantifier, one has stipulated that it should carve at the joints, and given Sider's metaontological framework, using such a quantifier warrants that one follow the method of contemporary analytic ontology when trying to discern what there is (Sider 2011, pp. 168–173). So we still need Ontologese, but aren't rationally required to speak it when describing what there is.

These remarks are insufficient as a complete account of how Sider's view of ontology interacts with quantifier variantism, and exactly where the rubber hits the road is still a little unclear. But given quantificational structure, it would seem that – depending on what that structure actually is like – it might not be the case that all the ontological languages Hirsch discuss are truth-conditionally equivalent after all. In that case, those who believe that there is quantificational structure have a powerful argument against quantifier variantism, for some languages are simply expressively stronger. In that case, this is an argument against weak quantifier variantism, and one doesn't have to rely on an s-norm. Truth is enough: Speakers of Ontologese (or some equally or more expansive language) are simply able to state all the truths, and speakers of some restricted languages are not.

On the other hand, if it turns out that we cannot explicate the betterness of Ontologese by it being better because it expresses more truths, as the above response suggests, then an s-

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<sup>148</sup> This is somewhat simplified. There might be other reasons for the necessity of using such a quantifier. For instance, Sider makes certain further claims about what's fundamental and what the fundamental is like. First, it is complete, i.e. every non-fundamental truth holds in virtue of some fundamental truth (Sider 2011, p. 115) and the fundamental is also supposed to be pure, i.e. fundamental truths involve only fundamental notions (Sider 2011, p. 115). Thus a fundamental language has all and only the structural concepts in its lexicon, and completely describes reality. The book of the world is written in a fundamental language. If all this is correct, then it would seem that a successful representation of reality must use structural concepts because the fundamental is pure and complete. The relationship between this rationale for using joint-carving concepts and the s-norm can unfortunately not be explored further here.

norm is necessary for Sider to provide a successful defense of ontology against quantifier variantism. What I hope to have shown is that as far as Sider's answer to quantifier variantism should be construed as a form of normative quantifier variantism, it is unsuccessful. Sider's arguments for quantificational structure are rather compelling, but whether his account ultimately succeeds as a response to Hirsch's arguments is a question for another day.

## 4.6 Concluding remarks

This chapter has focused on arguing against the s-norm. The s-norm seems to be an important part of Sider's defense of ontology against quantifier variantism, and it is also an aspect of the theory that is rather unexplored in the literature so far. In section 4.4, I argued that the arguments Sider provides for the s-norm don't justify postulating it, as it isn't necessary to use structural terms to talk about structure. This undermines the defense of ontology that it is most natural to take Sider to be advocating. But as we saw in section 4.5, Sider's defense of ontology might go through even without the s-norm. In any case, I'm positive that this isn't the last that is written on Sider's defense of ontology. The general theory that it is part of is complex, subtle and powerful, and there may be even more routes to a defense of ontology implicit in the theory than I've been able to see.

In closing, I wish to draw attention to a potential general problem with Sider's defense against quantifier variantism. As I mentioned in section 3.1, many of the questions that ontology seeks to answer arise in plain old English. The paradoxes of persistence and the problems of composition formulated in *English* don't go away if we migrate our ontological disagreements to Ontologese. For this reason, I find a defense of ontology against Hirsch along the lines of chapters 2 and 3 more satisfactory, for the ontological problems arising in English presumably constitute the reason to engage in ontology in the first place, and I would still want to see them solved. On the other hand, Sider will perhaps claim that ontology in English should be understood as the metasemantic task of determining the meaning of our words, and that English is therefore irrelevant to discerning what there is (this seems suggested in Sider 2011, p. 172). As indicated, I for my part am not yet ready to leave English for the promised land of Ontologese.

## 5 CONCLUSION

In this thesis, I have discussed Eli Hirsch's deflationary arguments against ontology. Chapter 1 provided a clear statement of Hirsch's arguments, paving the way for the evaluation of them in later chapters. We saw that the argument for quantifier variantism crucially hinges on a notion of "truth-conditionally equivalent" languages, and that the argument from charity consists of a detailed interpretation of the linguistic behavior of ontologists.

In chapter 2 I argued that the consequences of quantifier variantism are more wide-ranging than commonly thought, and that in order to avoid the semantic arguments of Eklund, Hawthorne and Harris, the quantifier variantist has to accept that different ontological languages have different semantic categories. But this eventually led to the conclusion (2.3) that predicates differ in meaning between ontological languages, undermining the deflationary conclusions of quantifier variantism, since there not being *F*s doesn't have bearing on whether there are *F*\*s (where "*F*" and "*F*\*" are different predicates). I know of no way to avoid predicate variantism, but even if the quantifier variantist should be able to find one, I also argued (2.4) that her position entails that mathematical terms have different meanings in different ontological languages – a rather uncomfortable consequence. I further argued (2.5) that Hirsch's notion of truth-conditional equivalence is hard to grasp, and that Hirsch has trouble accounting for the fact that ontologists often disagree about what's possible. Chapter 2 thus considered several foundational questions concerning quantifier variantism, and formulated some important objections that the quantifier variantist must address.

In chapter 3, the argument from charity came under scrutiny. I argued that ontologists should be taken to speak English, but that Hirsch's argument to the effect that denials of common sense ontological claims are "trivially false" in English is too quick. We can resist this argument when we realize that it is far from clear that perception gives justification for people's belief in ordinary objects. The upshot is that the argument from charity does not stand in the way of ontology conducted in English. There are of course *other* problems with revisionary ontology not addressed in this thesis. Ontology is difficult, and we shouldn't expect the disputes to be resolved any time soon, but I hope this thesis provides some ways for ontologists to move on from Hirsch's attack, and eventually get back to ontology.

Finally, in chapter 4 I considered an alternative way to respond to Hirsch's arguments, due to Ted Sider (2011). He argues that if we want to engage in substantive ontology, we must migrate ontological disputes from English to the dedicated ontological language Ontologese. I argued that his version of normative quantifier variantism fails, because it depends on an implausible epistemic norm. While this might not be the last word on the matter (4.5), I believe (1) that quantifier variantism is false, and (2) that my argument against Hirsch's argument from charity shows that ontology in English is possible. Thus ontologists need not hang their hopes on Sider's Ontologese – perhaps they can continue their inquiry in plain old English.

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